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CONTENTS

Conditioned Emotional Reactions: John B. Watson and Rosalie Rayner, 1.
The Organic Effects of Repeated Bodily Rotation: Coleman R. Griffith, 15.
A Series of Form Boards: George Oscar Ferguson, 47.
An Apparatus for Determining Acuity at Low Illuminations, for Testing the Light and Color Sense and for Detecting Small Errors in Refraction and in Their Correction: C. E. Ferree and Gertrude Rahn, 59.
Reaction Time Symptoms of Deception: W. Marston, 72.

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PSYCHOLOGICAL REVIEW

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CONTENTS OF VOLUME III

February.

Conditioned Emotional Reactions. JOHN B. WATSON and ROSALIE RAYNER, 1.
The Organic Effects of Repeated Bodily Rotation. COLEMAN R. GRIFFITH, 15.
A Series of Form Boards. GEORGE OSCAR FERGUSON, 47.
An Apparatus for Determining Acuity at Low Illumination, for Testing the Light and Color Sense and for Detecting Small Errors in Refraction and in Their Correlation. C. E. FERREE and GERTRUDE RAND, 59.
Reaction Time Symptoms of Deception. W. MARSTON, 72.

April.

An Experimental Study of Dizziness. COLEMAN R. GRIFFITH, 89.
Interdependence of Judgments Within the Series for the Method of Constant Stimuli. SAMUEL W. FERNBERGER, 126.

Discussion

Note on Mr. G. J. Rich's 'Study of Tonal Attributes.' HENRY J. WATT, 151.
Reply to Dr. Watt's "Note." GILBERT J. RICH, 155.

June.

The Influence of the Group upon Association and Thought. FLOYD H. ALLPORT, 159.
Attributive vs. Cognitive Clearness. KARL M. DALLENBACH, 183.
Curves of Growth of Intelligence. HERBERT A. TOOPS and RUDOLF PINTNER, 231.

August.

The Use of the Illumination Scale for the Detection of Small Errors in Refractive and in their Correction. C. E. FERREE and GERTRUDE RAND, 243.
The Backward Elimination of Errors in Mental Maze Learning. JOSEPH PETERSON, 257.
Some Volitional Patterns Revealed by the Will-profile. JUNE E. DOWNEY, 281.
On the 'After-Sensation' of Pressure. R. T. HOLLAND, 302.

October.

The Effect of Fatigue on Attention. JOHN J. B. MORGAN, 319.
Effects of Smoking on Mental and Motor Efficiency. SVEN FROEBERG, 334.
A Study of Ocular Functions with Special reference to the Lookout and Signal Service of the Navy. C. E. FERREE, G. RAND and D. BUCKLEY, 347.
The Block-Design Tests. S. C. KOHS, 357.
Recent Apparatus from the Psychological Laboratory of McLean Hospital. F. L. WELLS and C. M. KELLEY, 377.
Sex Differences in the Effect of Discussion. HAROLD E. BURTT, 390.
Practice Effects in Intelligence Tests. KNIGHT DUNLAP and AGNES SNYDER, 396.

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December.

Psychological Tests for Selecting Aviators. GEORGE M. STRATTON, HENRY C. MCCOMAS, JOHN E. COOVER and ENGLISH BAGBY, 405.

The Relative Value of Grouped and Interspersed Recitations. E. B. SKAGGS, 424.

Results of Some Experiments on Affection, Distribution of Associations, and Recall. C. H. GRIFFITTS, 447.

Two New Time Control Instruments. DONALD A. LAIRD, 465.

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CONDITIONED EMOTIONAL REACTIONS

BY JOHN B. WATSON AND ROSALIE RAYNER

In recent literature various speculations have been entered into concerning the possibility of conditioning various types of emotional response, but direct experimental evidence in support of such a view has been lacking. If the theory advanced by Watson and Morgan[1] to the effect that in infancy the original emotional reaction patterns are few, consisting so far as observed of fear, rage and love, then there must be some simple method by means of which the range of stimuli which can call out these emotions and their compounds is greatly increased. Otherwise, complexity in adult response could not be accounted for. These authors without adequate experimental evidence advanced the view that this range was increased by means of conditioned reflex factors. It was suggested there that the early home life of the child furnishes a laboratory situation for establishing conditioned emotional responses. The present authors have recently put the whole matter to an experimental test.

Experimental work has been done so far on only one child, Albert B. This infant was reared almost from birth in a hospital environment; his mother was a wet nurse in the Harriet Lane Home for Invalid Children. Albert's life was normal: he was healthy from birth and one of the best developed youngsters ever brought to the hospital, weighing twenty-one pounds at nine months of age. He was on the whole stolid and unemotional. His stability was one of the principal reasons for using him as a subject in this test. We

[1] 'Emotional Reactions and Psychological Experimentation,' *American Journal of Psychology*, April, 1917, Vol. 28, pp. 163–174.

felt that we could do him relatively little harm by carrying out such experiments as those outlined below.

At approximately nine months of age we ran him through the emotional tests that have become a part of our regular routine in determining whether fear reactions can be called out by other stimuli than sharp noises and the sudden removal of support. Tests of this type have been described by the senior author in another place.[1] In brief, the infant was confronted suddenly and for the first time successively with a white rat, a rabbit, a dog, a monkey, with masks with and without hair, cotton wool, burning newspapers, etc. A permanent record of Albert's reactions to these objects and situations has been preserved in a motion picture study. Manipulation was the most usual reaction called out. *At no time did this infant ever show fear in any situation.* These experimental records were confirmed by the casual observations of the mother and hospital attendants. No one had ever seen him in a state of fear and rage. The infant practically never cried.

Up to approximately nine months of age we had not tested him with loud sounds. The test to determine whether a fear reaction could be called out by a loud sound was made when he was eight months, twenty-six days of age. The sound was that made by striking a hammer upon a suspended steel bar four feet in length and three-fourths of an inch in diameter. The laboratory notes are as follows:

> One of the two experimenters caused the child to turn its head and fixate her moving hand; the other, stationed back of the child, struck the steel bar a sharp blow. The child started violently, his breathing was checked and the arms were raised in a characteristic manner. On the second stimulation the same thing occurred, and in addition the lips began to pucker and tremble. On the third stimulation the child broke into a sudden crying fit. This is the first time an emotional situation in the laboratory has produced any fear or even crying in Albert.

[1] 'Psychology from the Standpoint of a Behaviorist,' p. 202.

We had expected just these results on account of our work with other infants brought up under similar conditions. It is worth while to call attention to the fact that removal of support (dropping and jerking the blanket upon which the infant was lying) was tried exhaustively upon this infant on the same occasion. It was not effective in producing the fear response. This stimulus is effective in younger children. At what age such stimuli lose their potency in producing fear is not known. Nor is it known whether less placid children ever lose their fear of them. This probably depends upon the training the child gets. It is well known that children eagerly run to be tossed into the air and caught. On the other hand it is equally well known that in the adult fear responses are called out quite clearly by the sudden removal of support, if the individual is walking across a bridge, walking out upon a beam, etc. There is a wide field of study here which is aside from our present point.

The sound stimulus, thus, at nine months of age, gives us the means of testing several important factors. I. Can we condition fear of an animal, *e.g.*, a white rat, by visually presenting it and simultaneously striking a steel bar? II. If such a conditioned emotional response can be established, will there be a transfer to other animals or other objects? III. What is the effect of time upon such conditioned emotional responses? IV. If after a reasonable period such emotional responses have not died out, what laboratory methods can be devised for their removal?

I. The establishment of conditioned emotional responses. At first there was considerable hesitation upon our part in making the attempt to set up fear reactions experimentally. A certain responsibility attaches to such a procedure. We decided finally to make the attempt, comforting ourselves by the reflection that such attachments would arise anyway as soon as the child left the sheltered environment of the nursery for the rough and tumble of the home. We did not begin this work until Albert was eleven months, three days of age. Before attempting to set up a conditioned response we, as before, put him through all of the regular emotional

tests. *Not the slightest sign of a fear response was obtained in any situation.*

The steps taken to condition emotional responses are shown in our laboratory notes.

11 Months 3 Days

1. White rat suddenly taken from the basket and presented to Albert. He began to reach for rat with left hand. Just as his hand touched the animal the bar was struck immediately behind his head. The infant jumped violently and fell forward, burying his face in the mattress. He did not cry, however.

2. Just as the right hand touched the rat the bar was again struck. Again the infant jumped violently, fell forward and began to whimper.

In order not to disturb the child too seriously no further tests were given for one week.

11 Months 10 Days

1. Rat presented suddenly without sound. There was steady fixation but no tendency at first to reach for it. The rat was then placed nearer, whereupon tentative reaching movements began with the right hand. When the rat nosed the infant's left hand, the hand was immediately withdrawn. He started to reach for the head of the animal with the forefinger of the left hand, but withdrew it suddenly before contact. It is thus seen that the two joint stimulations given the previous week were not without effect. He was tested with his blocks immediately afterwards to see if they shared in the process of conditioning. He began immediately to pick them up, dropping them, pounding them, etc. In the remainder of the tests the blocks were given frequently to quiet him and to test his general emotional state. They were always removed from sight when the process of conditioning was under way.

2. Joint stimulation with rat and sound. Started, then fell over immediately to right side. No crying.

3. Joint stimulation. Fell to right side and rested upon hands, with head turned away from rat. No crying.

4. Joint stimulation. Same reaction.

5. Rat suddenly presented alone. Puckered face, whimpered and withdrew body sharply to the left.

6. Joint stimulation. Fell over immediately to right side and began to whimper.

7. Joint stimulation. Started violently and cried, but did not fall over.

8. Rat alone. *The instant the rat was shown the baby began to cry. Almost instantly he turned sharply to the left, fell over on left side, raised himself on all fours and began to crawl away so rapidly that he was caught with difficulty before reaching the edge of the table.*

This was as convincing a case of a completely conditioned fear response as could have been theoretically pictured. In all seven joint stimulations were given to bring about the complete reaction. It is not unlikely had the sound been of greater intensity or of a more complex clang character that the number of joint stimulations might have been materially reduced. Experiments designed to define the nature of the sounds that will serve best as emotional stimuli are under way.

II. When a conditioned emotional response has been established for one object, is there a transfer? Five days later Albert was again brought back into the laboratory and tested as follows:

11 Months 15 Days

1. Tested first with blocks. He reached readily for them, playing with them as usual. This shows that there has been no general transfer to the room, table, blocks, etc.

2. Rat alone. Whimpered immediately, withdrew right hand and turned head and trunk away.

3. Blocks again offered. Played readily with them, smiling and gurgling.

4. Rat alone. Leaned over to the left side as far away from the rat as possible, then fell over, getting up on all fours and scurrying away as rapidly as possible.

5. Blocks again offered. Reached immediately for them, smiling and laughing as before.

The above preliminary test shows that the conditioned response to the rat had carried over completely for the five days in which no tests were given. The question as to whether or not there is a transfer was next taken up.

6. Rabbit alone. The rabbit was suddenly placed on the mattress in front of him. The reaction was pronounced. Negative responses began at once. He leaned as far away from the animal as possible, whimpered, then burst into tears. When the rabbit was placed in contact with him he buried his face in the mattress, then got up on all fours and crawled away, crying as he went. This was a most convincing test.

7. The blocks were next given him, after an interval. He played with them as before. It was observed by four people that he played far more energetically with them than ever before. The blocks were raised high over his head and slammed down with a great deal of force.

8. Dog alone. The dog did not produce as violent a reaction as the rabbit. The moment fixation occurred the child shrank back and as the animal came nearer he attempted to get on all fours but did not cry at first. As soon as the dog passed out of his range of vision he became quiet. The dog was then made to approach the infant's head (he was lying down at the moment). Albert straightened up immediately, fell over to the opposite side and turned his head away. He then began to cry.

9. The blocks were again presented. He began immediately to play with them.

10. Fur coat (seal). Withdrew immediately to the left side and began to fret. Coat put close to him on the

left side, he turned immediately, began to cry and tried to crawl away on all fours.

11. Cotton wool. The wool was presented in a paper package. At the end the cotton was not covered by the paper. It was placed first on his feet. He kicked it away but did not touch it with his hands. When his hand was laid on the wool he immediately withdrew it but did not show the shock that the animals or fur coat produced in him. He then began to play with the paper, avoiding contact with the wool itself. He finally, under the impulse of the manipulative instinct, lost some of his negativism to the wool.

12. Just in play W. put his head down to see if Albert would play with his hair. Albert was completely negative. Two other observers did the same thing. He began immediately to play with their hair. W. then brought the Santa Claus mask and presented it to Albert. He was again pronouncedly negative.

11 Months 20 Days

1. Blocks alone. Played with them as usual.

2. Rat alone. Withdrawal of the whole body, bending over to left side, no crying. Fixation and following with eyes. The response was much less marked than on first presentation the previous week. It was thought best to freshen up the reaction by another joint stimulation.

3. Just as the rat was placed on his hand the rod was struck. Reaction violent

4. Rat alone. Fell over at once to left side. Reaction practically as strong as on former occasion but no crying.

5. Rat alone. Fell over to left side, got up on all fours and started to crawl away. On this occasion there was no crying, but strange to say, as he started away he began to gurgle and coo, even while leaning far over to the left side to avoid the rat.

6. Rabbit alone. Leaned over to left side as far as possible. Did not fall over. Began to whimper but reaction not so violent as on former occasions.

7. Blocks again offered. He reached for them immediately and began to play.

All of the tests so far discussed were carried out upon a table supplied with a mattress, located in a small, well-lighted dark-room. We wished to test next whether conditioned fear responses so set up would appear if the situation were markedly altered. We thought it best before making this test to freshen the reaction both to the rabbit and to the dog by showing them at the moment the steel bar was struck. It will be recalled that this was the first time any effort had been made to directly condition response to the dog and rabbit. The experimental notes are as follows:

8. The rabbit at first was given alone. The reaction was exactly as given in test (6) above. When the rabbit was left on Albert's knees for a long time he began tentatively to reach out and manipulate its fur with forefingers. While doing this the steel rod was struck. A violent fear reaction resulted.

9. Rabbit alone. Reaction wholly similar to that on trial (6) above.

10. Rabbit alone. Started immediately to whimper, holding hands far up, but did not cry. Conflicting tendency to manipulate very evident.

11. Dog alone. Began to whimper, shaking head from side to side, holding hands as far away from the animal as possible.

12. Dog and sound. The rod was struck just as the animal touched him. A violent negative reaction appeared. He began to whimper, turned to one side, fell over and started to get up on all fours.

13. Blocks. Played with them immediately and readily.

On this same day and immediately after the above experiment Albert was taken into the large well-lighted lecture room belonging to the laboratory. He was placed on a table in the center of the room immediately under the skylight. Four people were present. The situation

was thus very different from that which obtained in the small dark room.

1. Rat alone. No sudden fear reaction appeared at first. The hands, however, were held up and away from the animal. No positive manipulatory reactions appeared.

2. Rabbit alone. Fear reaction slight. Turned to left and kept face away from the animal but the reaction was never pronounced.

3. Dog alone. Turned away but did not fall over. Cried. Hands moved as far away from the animal as possible. Whimpered as long as the dog was present.

4. Rat alone. Slight negative reaction.

5. Rat and sound. It was thought best to freshen the reaction to the rat. The sound was given just as the rat was presented. Albert jumped violently but did not cry.

6. Rat alone. At first he did not show any negative reaction. When rat was placed nearer he began to show negative reaction by drawing back his body, raising his hands, whimpering, etc.

7. Blocks. Played with them immediately.

8. Rat alone. Pronounced withdrawal of body and whimpering.

9. Blocks. Played with them as before.

10. Rabbit alone. Pronounced reaction. Whimpered with arms held high, fell over backward and had to be caught.

11. Dog alone. At first the dog did not produce the pronounced reaction. The hands were held high over the head, breathing was checked, but there was no crying. Just at this moment the dog, which had not barked before, barked three times loudly when only about six inches from the baby's face. Albert immediately fell over and broke into a wail that continued until the dog was removed. The sudden barking of the hitherto quiet dog produced a marked fear response in the adult observers!

From the above results it would seem that emotional transfers do take place. Furthermore it would seem that the number of transfers resulting from an experimentally produced conditioned emotional reaction may be very large. In our observations we had no means of testing the complete number of transfers which may have resulted.

III. The effect of time upon conditioned emotional responses. We have already shown that the conditioned emotional response will continue for a period of one week. It was desired to make the time test longer. In view of the imminence of Albert's departure from the hospital we could not make the interval longer than one month. Accordingly no further emotional experimentation was entered into for thirty-one days after the above test. During the month, however, Albert was brought weekly to the laboratory for tests upon right and left-handedness, imitation, general development, etc. No emotional tests whatever were given and during the whole month his regular nursery routine was maintained in the Harriet Lane Home. The notes on the test given at the end of this period are as follows:

1 Year 21 Days

1. Santa Claus mask. Withdrawal, gurgling, then slapped at it without touching. When his hand was forced to touch it, he whimpered and cried. His hand was forced to touch it two more times. He whimpered and cried on both tests. He finally cried at the mere visual stimulus of the mask.

2. Fur coat. Wrinkled his nose and withdrew both hands, drew back his whole body and began to whimper as the coat was put nearer. Again there was the strife between withdrawal and the tendency to manipulate. Reached tentatively with left hand but drew back before contact had been made. In moving his body to one side his hand accidentally touched the coat. He began to cry at once, nodding his head in a very peculiar manner (this reaction was an entirely new one). Both hands were withdrawn as far as possible from the coat. The coat

was then laid on his lap and he continued nodding his head and whimpering, withdrawing his body as far as possible, pushing the while at the coat with his feet but never touching it with his hands.

3. Fur coat. The coat was taken out of his sight and presented again at the end of a minute. He began immediately to fret, withdrawing his body and nodding his head as before.

4. Blocks. He began to play with them as usual.

5. The rat. He allowed the rat to crawl towards him without withdrawing. He sat very still and fixated it intently. Rat then touched his hand. Albert withdrew it immediately, then leaned back as far as possible but did not cry. When the rat was placed on his arm he withdrew his body and began to fret, nodding his head. The rat was then allowed to crawl against his chest. He first began to fret and then covered his eyes with both hands.

6. Blocks. Reaction normal.

7. The rabbit. The animal was placed directly in front of him. It was very quiet. Albert showed no avoiding reactions at first. After a few seconds he puckered up his face, began to nod his head and to look intently at the experimenter. He next began to push the rabbit away with his feet, withdrawing his body at the same time. Then as the rabbit came nearer he began pulling his feet away, nodding his head, and wailing "da da." After about a minute he reached out tentatively and slowly and touched the rabbit's ear with his right hand, finally manipulating it. The rabbit was again placed in his lap. Again he began to fret and withdrew his hands. He reached out tentatively with his left hand and touched the animal, shuddered and withdrew the whole body. The experimenter then took hold of his left hand and laid it on the rabbit's back. Albert immediately withdrew his hand and began to suck his thumb. Again the rabbit was laid in his lap. He began to cry, covering his face with both hands.

8. Dog. The dog was very active. Albert fixated it intensely for a few seconds, sitting very still. He began to cry but did not fall over backwards as on his last contact with the dog. When the dog was pushed closer to him he at first sat motionless, then began to cry, putting both hands over his face.

These experiments would seem to show conclusively that directly conditioned emotional responses as well as those conditioned by transfer persist, although with a certain loss in the intensity of the reaction, for a longer period than one month. Our view is that they persist and modify personality throughout life. It should be recalled again that Albert was of an extremely phlegmatic type. Had he been emotionally unstable probably both the directly conditioned response and those transferred would have persisted throughout the month unchanged in form.

IV. "Detachment" or removal of conditioned emotional responses. Unfortunately Albert was taken from the hospital the day the above tests were made. Hence the opportunity of building up an experimental technique by means of which we could remove the conditioned emotional responses was denied us. Our own view, expressed above, which is possibly not very well grounded, is that these responses in the home environment are likely to persist indefinitely, unless an accidental method for removing them is hit upon. The importance of establishing some method must be apparent to all. Had the opportunity been at hand we should have tried out several methods, some of which we may mention. (1) Constantly confronting the child with those stimuli which called out the responses in the hopes that habituation would come in corresponding to "fatigue" of reflex when differential reactions are to be set up. (2) By trying to "recondition" by showing objects calling out fear responses (vsual) and simultaneously stimulating the erogenous zones (tactual). We should try first the lips, then the nipples and as a final resort the sex organs. (3) By trying to "recondition" by feeding the subject candy or other food just as the animal is shown. This method calls for the food control of the subject. (4) By building up "constructive" activities around the object by imitation and

by putting the hand through the motions of manipulation. At this age imitation of overt motor activity is strong, as our present but unpublished experimentation has shown.

Incidental Observations

(a) Thumb sucking as a compensatory device for blocking fear and noxious stimuli. During the course of these experiments, especially in the final test, it was noticed that whenever Albert was on the verge of tears or emotionally upset generally he would continually thrust his thumb into his mouth. The moment the hand reached the mouth he became impervious to the stimuli producing fear. Again and again while the motion pictures were being made at the end of the thirty-day rest period, we had to remove the thumb from his mouth before the conditioned response could be obtained. This method of blocking noxious and emotional stimuli (fear and rage) through erogenous stimulation seems to persist from birth onward. Very often in our experiments upon the work adders with infants under ten days of age the same reaction appeared. When at work upon the adders both of the infants arms are under slight restraint. Often rage appears. They begin to cry, thrashing their arms and legs about. If the finger gets into the mouth crying ceases at once. The organism thus apparently from birth, when under the influence of love stimuli is blocked to all others.[1] This resort to sex stimulation when under the influence of noxious and emotional situations, or when the individual is restless and idle, persists throughout adolescent and adult life. Albert, at any rate, did not resort to thumb sucking except in the presence of such stimuli. Thumb sucking could immediately be checked by offering him his blocks. These invariably called out active manipulation instincts. It is worth while here to call attention to the fact that Freud's conception of the stimulation of erogenous zones as being the expression of an original "pleasure" seeking principle may be turned about

[1] The stimulus to love in infants according to our view is stroking of the skin, lips, nipples and sex organs, patting and rocking, picking up, etc. Patting and rocking (when not conditioned) are probably equivalent to actual stimulation of the sex organs. In adults of course, as every lover knows, vision, audition and olfaction soon become conditioned by joint stimulation with contact and kinæsthetic stimuli.

and possibly better described as a compensatory (and often conditioned) device for the blockage of noxious and fear and rage producing stimuli.

(b) Equal primacy of fear, love and possibly rage. While in general the results of our experiment offer no particular points of conflict with Freudian concepts, one fact out of harmony with them should be emphasized. According to proper Freudians sex (or in our terminology, love) is the principal emotion in which conditioned responses arise which later limit and distort personality. We wish to take sharp issue with this view on the basis of the experimental evidence we have gathered. Fear is as primal a factor as love in influencing personality. Fear does not gather its potency in any derived manner from love. It belongs to the original and inherited nature of man. Probably the same may be true of rage although at present we are not so sure of this.

The Freudians twenty years from now, unless their hypotheses change, when they come to analyze Albert's fear of a seal skin coat—assuming that he comes to analysis at that age—will probably tease from him the recital of a dream which upon their analysis will show that Albert at three years of age attempted to play with the pubic hair of the mother and was scolded violently for it. (We are by no means denying that this might in some other case condition it). If the analyst has sufficiently prepared Albert to accept such a dream when found as an explanation of his avoiding tendencies, and if the analyst has the authority and personality to put it over, Albert may be fully convinced that the dream was a true revealer of the factors which brought about the fear.

It is probable that many of the phobias in psychopathology are true conditioned emotional reactions either of the direct or the transferred type. One may possibly have to believe that such persistence of early conditioned responses will be found only in persons who are constitutionally inferior. Our argument is meant to be constructive. Emotional disturbances in adults cannot be traced back to sex alone. They must be retraced along at least three collateral lines—to conditioned and transferred responses set up in infancy and early youth in all three of the fundamental human emotions.

THE ORGANIC EFFECTS OF REPEATED BODILY ROTATION[1]

BY COLEMAN R. GRIFFITH

TABLE OF CONTENTS

I. Introduction 15
A. Historical setting 16
B. Statement of problem 18
C. Discussion of methods 19
II. The experiments 20
A. The method of periodic rotation 20
B. The general procedure 20
C. The subjects 24
III. The organic results of rotation 25
A. Local effects: nystagmus 25
B. Systemic effects: muscular innervations: visceral and vascular changes 26
IV. Physical and physiological factors which modify the organic results of rotation 27
A. Periodic repetition 27
B. Physical conditions 37
1. Speed of rotation 38
2. Mode of stopping 38
3. Reversal of direction 38
C. Temporal conditions 39
1. Time of day 39
2. Effect of intervals 40
D. General organic state 42
E. Modification by 'transfer' 44
V. Conclusions 46

I. INTRODUCTION

It has been frequently observed that a rapid spin upon the heel or a few turns on a merry-go-round may lead to a confused and uncomfortable state of mind and to an uncertain control of the body. So sensitive is the organism to disturbances of this kind that a quick jerk of the head to the right and to the left is often sufficient to induce a momentary giddiness; while under more violent conditions, the effects may be severe and widespread.

[1] From the Psychological Laboratory of the University of Illinois.

A. Historical Setting.—Scientific interest in the nature and the causes of dizziness and of the concomitant loss of bodily control goes back almost a hundred years.[1]

About that time the discovery was made by Flourens[2] that this kind of disturbance could be induced by exciting certain portions of the ear or by surgical insults of one kind and another to the cerebellum and to other central structures. The facts of dizziness had already been admirably described by Darwin[3] and Purkinje[4]; and the relation of the ear to these facts raised a good deal of discussion, for it had been commonly supposed that the ear was concerned with hearing alone. But experimental evidence soon made it clear that a part of the internal ear was not in any way connected with the mediation of auditory qualities, and with this demonstration a new group of problems was proposed to experimental physiology and psychology. It was first necessary clearly to establish the precise relation obtaining between the excitation of the internal ear and the effects incident to a disturbed balance. As a result of the painstaking work of a large number of investigators this relation was established in such a manner as to bring about the discovery of a new end-organ in the semicircular canals, *viz.*, the hair cells in the ampullæ. With this discovery, investigators went on to describe the mode of excitation of the new end-organ, to delineate its central connections, and to analyse the effects produced by its excitation. Notable progress was made in spite of theorists who from time to time insisted that the canals were the end-organs for the appreciation of time and space, or for noise, or for apprehending the direction of sound. By the year 1900, three hundred experimental investigations

[1] A detailed survey of scientific literature on the semicircular canals based on a review of some seven hundred references, together with a bibliography of over a thousand titles, will presently be published.

[2] Flourens, P., 'Recherches expérimentales sur les propriétés et les fonctions du système nerveux.' Bailliere, Paris, 1842, pp. 438–501.

[3] Darwin, E., 'Zoönomia, the Laws of Organic Life,' 1795. 3d ed., 1801, pp. 327–356.

[4] Purkinje, J., 'Mitteilungen über Scheinbewegungen und über den Schwindel,' *Bull. d. naturw. Sektion d. Schles. Gesellsch. f. vaterland. Kultur*, 1825–26, 10. Reprinted in Aubert's 'Studien über die Orientierung,' Tübingen, 1888.

and more had accumulated a considerable body of doctrine about the canals and their functions. For example, it was maintained that the inertia of the liquid within the membranous labyrinth or even the inertia of the whole membranous structures provided the adequate stimulus for the vestibular end-organs. The excitation initiated was supposed to set up, through the central connections of the vestibular nerve, *i.e.*, the vestibular nucleus and the cerebellum, all kinds of equilibratory adjustments. Some of these were, according to Ewald, no more than variations in tonic effect, while many other investigators insisted that the adjustments were more positive, taking the form of movements compensatory to bodily displacement. In short, a great amount of circumstantial evidence had been accumulated going to show that the structures in the semicircular canals were *end-organs* concerned chiefly in the maintenance of equilibrium. Furthermore, it had come to be commonly accepted that balance or the maintenance of position was reflexly carried out by excitations from these end-organs.

Now chief,—because regular in appearance and easy of control,—among the movements excited by the stimulation of the semicircular canals, are certain ocular movements known as 'nystagmus.' In fact, these ocular movements are so constant in their appearance as to have won for themselves the designation of 'simple reflexes.' It is upon these movements that most of the recent work has been done, the shift in point of interest occurring in the following manner. About 1906 Robert Bárány of Vienna recognized the clinical significance of such a close relation between the ear and the eyes and he immediately began, therefore, to make clinical use of the great amount of work that had been done on the canals.[1] And thus, as is common in the history of the sciences, attention shifted from the question as purely academic to the practical application of the facts already at hand. It was Bárány's hope to use the ocular effects of vestibular stimulation as an index to the functional integrity of the neural

[1] Bárány, R., 'Untersuchungen über den vom Vestibularapparate des Ohres reflektorisch ausgelösten rhythmischen Nystagmus und seine Begleiterscheinungen,' *Monatschr. f. Ohrenhk.*, 1906, 40, 193–297.

paths involved, especially of the paths lying within the cerebellum itself. One of the contemporary scientific problems is, therefore, directed toward the nature of these ocular effects of stimulation of the canals. In this connection, it is significant to note that almost, if not all, of the evidence for the function of the canals rests upon unrepeated excitations of various parts of the mechanisms concerned. That is to say, the stimulation of the end-organs in the semicircular canals results immediately in a complex group of organic and mental effects. Certain of these effects, and especially the ocular movements, regarded as reflex, have been put to important use in the clinical laboratory. No consistent account has been taken of the effect of continued excitation, either upon the ocular movements or upon the rest of the body.

B. Statement of Problem.—The problem of this investigation, therefore, is to describe the organic effects of repeated bodily rotation. It was observed by many of the earlier investigators that, when the vestibular branch of the eighth nerve was cut, or one or more of the canals injured, the compensatory movements either of the eyes or of the head and the changes in posture were at first intense but gradually disappeared within a few days. It is strange that this fact did not suggest that continued or repeated excitation of the end-organs in the canals would lead to a profound modification in the effects usually produced. As a matter of fact, however, we have failed to find an instance where a consistent effort has been made to repeat ampullar stimulation in the same organism a great number of times. This failure is partially due, no doubt, to the fact that almost all of the earlier work on the subject involved surgical means of excitation, a method which naturally excludes extensive repetition. Furthermore, the immediate effects are so complicated and then appear in such different forms in different organisms that clear thinking and a concensus of opinion have been almost impossible. We shall, therefore, undertake to excite the vestibular end-organs in the human subject a large number of times in succession and then seek to describe the organic effects issuing from this repeated stimulation.

C. Discussion of Methods.—Various methods are used in the clinical laboratory for the arousal of vestibular activity. The method of rotation was first used by Purkinje, and almost every investigator since his time has used it in one form or another. Bárány adapted it, as well as other methods, to the clinical laboratory. The method of rotation commends itself as preferable to other methods by the ease with which it can be controlled and also by reason of its adaptability.

The galvanic method has likewise had a long history. Purkinje,[1] Remak,[2] Brenner,[3] Hitzig,[4] Kny,[5] Breuer,[6] Jensen,[7] and many others used it for the excitation of 'vestibular sensations.' Of these investigators, Hitzig was the first to give a clear and accurate account of 'galvanic vertigo.' (op. cit., p. 716ff.) Jones[8] describes the recent use of the galvanic method; but the method has not been found so practicable as other methods. The thermal or caloric method has had a similar history. Goltz[9] said in 1870 that the effects of the use of hot and cold water had become common knowledge by that time. Hitzig,[10] Breuer,[11] and Bornhardt[12] have used the caloric method extensively. In case the tympanic membrane is broken, it is possible to excite the canals by pressure.

[1] Purkinje, J., 'Ueber die physiologische Bedeutung des Schwindels und die Beziehung derselben zu den neuesten Versuchen über die Hirnfunction,' *Rust's Mag. f. d. ges. Heilk.*, 1827, 23, 284–310.

[2] Remak, R., 'Galvanotherapie der Nerven und Muskelkrankheiten,' Berlin, 1858.

[3] Brenner, R., 'Untersuchungen und Beobachtungen auf dem Gebiete des Elektrotherapie,' Leipzig, 1868, I, 75ff; II, 30ff.

[4] Hitzig, E., 'Ueber die beim Galvanisieren des Kopfes enstehenden Störungen der Muskelinnervation und der Vorstellungen vom Verhalten im Raume,' *Arch. f. Anat. u. Physiol.*, 1871, 716–772.

[5] Kny, E., 'Untersuchungen über den galvanischen Schwindel,' *Arch. f. Psychiat.*, 1887, 18, 637–658.

[6] Breuer, J., 'Neue Versuche an den Ohrbogengängen,' *Pflüger's Arch. f. d. ges. Physiol.*, 1888, 44, 87–156.

[7] Jensen, P., 'Ueber den galvanischen Schwindel,' *Pflüger's Arch. f. d. ges. Physiol.*, 1896, 64, 182–222.

[8] Jones, I. H., 'Equilibrium and Vertigo,' 1918, pp. 247ff.

[9] Goltz, F., 'Ueber die physiologische Bedeutung der Bogengänge des Ohrlabyrinths,' *Pflüger's Arch. f. d. ges. Physiol.*, 1870, 3, 172–192.

[10] Hitzig, E., *op. cit.*

[11] Breuer, J., *op. cit.*

[12] Bornhardt, A., 'Experimentelle Beiträge zur Physiologie der Bogengänge des Ohrlabyrinths,' *Pflüger's Arch. f. d. ges. Physiol.*, 1876, 12, 471–521.

Alexander,[1] after describing the methods we are discussing, laid claim to the first use of the method of pressure; but Bárány[2] says that several investigators had observed the effects of pressure before Alexander. Mechanical stimulation with needles and by cutting as well as chemical stimulation have frequently been resorted to. Spamer[3] used all of these methods with keen critical ability. From this account, it is apparent, therefore, that Bárány's sole contribution to method has been the adaptation of these historical methods to the clinical laboratory.

II. The Experiments

A. The Method of Periodic Rotation.—Of the various means of exciting the canals which we have just discussed, we shall be concerned alone with the method of rotation. We shall, moreover, insist that the periodic use of this method is opposed in spirit and in practice to the method of single excitations and to observations of the immediate effects of such excitations. We shall find that the use of the method throws an entirely different light on certain present conceptions of the nature both of the organic and of the mental effects of ampullar stimulation. In our experiments, each subject was given a rotation-series once a day (Sundays and a few unavoidable lapses excepted), a series comprising five trials to the right and five to the left, given alternately (*i. e.*, one trial to the right and then one to the left, and so on) and each trial consisting of ten revolutions. For the most part, one minute's rest was given between trials, with three minutes between every five trials. Three or four subjects were used simultaneously, the series being continued in each case so long as the subject was available.

B. The General Procedure.—For the purposes of our experiments, a modified Bárány rotating chair was used. This chair, which is described by Jones,[4] has suffered certain

[1] Alexander, G., 'Die Funktion des Vestibularapparates,' *Bericht u. d. IV. Kongress f. exper. Psychol.*, 1911, 74–94.

[2] Bárány, R., see Alexander, G., *op. cit.*, p. 91.

[3] Spamer, C., 'Experimenteller und kritischer Beitrag zur Physiologie des halbzirkelförmigen Kanäle,' *Pflüger's Arch. f. d. ges. Physiol.*, 1880, 21, 479–590.

[4] Jones, I. H., *op. cit.*, pp. 233ff.

American modifications but these are of doubtful value for research. The chair used in our experiments was especially designed for research by Professor Bentley.

In the first place, the chair was so constructed as to enable the operator to stop it gradually by a friction-brake rather than suddenly as in certain clinical models. The reason for and significance of this provision will presently appear. The chair was also insulated and wired in such a manner that the observer could graphically register, by the pressure of a key placed in his hand, either during or after rotation, any time intervals or events which he might from time to time be instructed to note and to record. In addition, contact points were adjusted at intervals of 90 degrees about the base of the chair so that its rate of revolution could be indicated on the smoked drum. By comparing the records from these contact points with the records made from a second's pendulum, it was possible to find that the chair was rotated with an average mean variation of less than one-tenth of a second per revolution.[1] By means of two projections, one in front and one at the back, an almost continuous impulsion was given to the chair. In this way, jarring and irregularities in the rate of rotation were so far reduced that observers repeatedly described the motion as similar to that of a well-balanced object along a smooth and oily surface.[2] A click from the second's pendulum and the visual cue from its swing gave to the experimenter a wholly dependable kinæsthetic and visual rhythm, making it possible to start and to stop the chair and to maintain its speed with a negligible degree of variation. The records of the smoked drum indicated that the chair attained full speed at the end of one second, the mean variation of error being less than one fifth of one second. The regular rate of rotation—one turn in two seconds for ten

[1] Dunlap asserts that the variation in the clinical laboratory has been as great as 13–27 sec. when a 20 sec. period was prescribed. See *J. Amer. Med. Ass.*, 1919, 73, 54.

[2] Absolute control of the rotation is vital if constancy is to be expected. The otologists have quite overlooked this factor, as is betrayed by the unevenness of the rotation and the abrupt stop. Several observers in this investigation who have taken the otological examination for Army Aviation declare that the rotation here given was far better controlled than that given during the official tests.

turns—was then maintained until the stop, which was accomplished evenly and with a mean variation of approximately one ninth of one second of error. A push-button connected with a magnetic signal marker enabled the experimenter to register on the smoked drum the number of eye-movements made after the chair came to rest. In a similar way was registered the time at which all apparent movement in the observer's visual field ceased. Two kinds of apparent movement in the visual field follow rotation. (1) An easily recognized jerky movement of objects, which is a function of the eye-movements described above, is followed (2) by a 'flowing' in the visual field which persists for several seconds. The visual field then clears up, save for whatever after-images may have been induced by the prolonged period of fixation. The disappearance of the jerky movements was alone considered in making our reports. (See Plate I, *a*, *b*.).

As regards the registration of the number of eye-movements, the following procedure was adopted. An Appunn lamella used for practice purposes served also to determine the accuracy of the experimenter in counting the number of eye-movements. Vibration-rates up to seven a second were accurately counted, and by counting every other vibration of the lamella the experimenter was able to record with accuracy rates up to twelve in the second, and considerably beyond with a small calculable error. The eye-movements are easier to record than are the vibrations of the lamella and, moreover, within the group of observers here reported, none showed an original rapidity of movement greater than five in the second. There were thus three values obtained from each subject as the series proceeded; (1) the subject's own record of the cessation of apparent movement of objects, (2) the record of the eye-movements made, and (3) of the duration of the after-nystagmus. Measurements of the amplitude of the movements were made with the aid of a reading microscope.

The method of treating these records can be seen in Table I, which is chosen at random from the original records. The table represents a single day's rotation-period of ten turns, five to the right and five to the left, done alternately. It can

be seen that for a decreasing group of measures the m.v.'s are relatively small. The regularity with which the first measures exceed and the last measures fall short of the average indicates throughout the investigation the regularity with which the decrease takes place. That is to say, these mean variations are, for the most part, an index of the regularity of decrease in the measures averaged and are not a true measure—as the m.v. should be—of reliability.

TABLE I

Trial	Rotation to Right						Rotation to Left					
	Time	M. V.	No.	M.V.	App. Mvt.	M.V.	Time	M.V	No.	M.V.	App. Mvt.	M.V.
1	19.0	2.8	38.0	1.6	19.0	2.6	20.0	3.4	39.0	2.8	22.0	4.8
2	16.0	0.2	39.0	2.6	17.0	0.6	18.0	1.4	39.0	2.8	18.0	0.8
3	16.0	0.2	37.0	0.6	16.0	0.4	16.0	0.6	37.0	0.8	17.0	0.2
4	17.0	0.8	34.0	2.4	16.0	0.4	15.0	1.6	35.0	1.2	15.0	2.2
5	13.0	3.2	34.0	2.4	14.0	2.4	14.0	2.6	31.0	5.2	14.0	3.2
Average	16.2	1.44	36.4	1.92	16.4	1.28	16.6	1.42	36.2	2.56	17.2	2.24
Aver. R. and L.							16.4	1.43	36.3	2.24	16.8	1.76

A fixation point was provided on a large black background upon which were placed vertical white strips 12 mm. wide and 25 mm. apart. The fixation point was a small cross in the center of the field set on one of the strips of white and was 1 m. from the eyes of the subject. These vertical strips enabled the subject to indicate the cessation of apparent movement with much more ease and accuracy than he could have done with the otologist's indefinite fixation outside a window.[1] The position of each observer was held constant by a rod adjusted to the side of the chair in such a way that it stood at an angle of 30 degrees from the vertical. The head of the individual was then placed in such a position—by the use of an improved head-rest—that a line connecting the middle of the shoulder with the middle of the ear was

[1] As a matter of fact, we shall show in a later paper that fixation is an important item in the conditions that govern the appearance of the organic as well as the mental effects of rotation. Fixation at some point out of a window as used by the otologists in the official tests of the Army Air Service, *e.g.*, is extremely inaccurate and undependable.

parallel with the rod. It was thus made certain that the head of the subject was always in such a constant position that the horizontal canals were approximately parallel with the floor and at right angles to the axis of rotation. After the 'ready' signal and at the 'now' signal, the subject was turned ten times in twenty seconds[1] and stopped by means of the brake so that a line on the foot-rest of the chair exactly coincided with the line placed on the floor. After a little practice, the experimenter found that this could be accomplished with an average error of less than 2 cm.

C. The Subjects.—The subjects used during these experiments were chosen at random from the various courses in psychology and were for the most part upper-classmen.[2] Before using them in the experiments, they were given the usual turning tests for 'vestibular normality.' That is to say, the clinical laboratory has been working on the assumption that if the ocular movements following rotation persist for an average time of 25 sec., in so far forth, the subject is 'normal.' Usually, however, further tests are given such as

[1] There is nothing sacred about this rate of rotation, although some of the otologists seem so to regard it. The appearance of the effects of rotation is directly proportional to the rate and the number of turns. That is to say, one revolution at any ordinary speed will not, as a rule, produce nystagmus, but two turns may. The amount of nystagmus then increases as the number of turns increases. The ratio has not been established and neither have the limits, although it is known that the duration of the after-nystagmus increases with a constant rate of rotation (1 rev. in 2 sec.) up to fifteen turns. Bárány found that the after-nystagmus suffers decided change in kind when the number of rotations is made as high as sixty. Ocular and other organic effects generally appear in an unpracticed individual after two complete turns. The speed of ten rotations in 20 sec. was chosen partly because it admitted adequate comparison of the results with those of other investigators and partly because the effects aroused are of sufficient intensity to furnish suitable conditions for observation and description. Any further increase in the intensity of the effects would tend to defeat the purpose of the experiment.

[2] The writer wishes to acknowledge his appreciation of the services of all the subjects who willingly submitted themselves to the trying conditions of the experiment. The names follow: V. B. Adams, H. C. Burleson, J. W. Cannon, L. K. Cecil, S. M. Dietrich, A. R. Elliott, W. H. Griffith, D. T. Harris, H. O. Hope, S. C. Nag, L. R. Raines, W. H. Rayner, J. A. Sanders, A. D. Sinden, H. M. VanDoren, R. W. Wuestermann. H. C. Burleson acted as experimenter while the writer was a subject. The writer wishes also to express his sincere appreciation of the aid and counsel of Professor M. Bentley, under whose direction and criticism this investigation was undertaken and carried through.

the 'past-pointing' test and the 'falling' test. Both of these tests depend upon systemic innervations from the end-organs in the canals. (See Jones, I. H., 'Equilibrium and Vertigo,' 1918, *passim*.) An otologist who had been selected by the Surgeon General's Office to give tests to candidates for the Air Service pronounced all our subjects 'normal' after reviewing our results and giving some of the tests himself. He assured us that our subjects gave an average after-nystagmus of acceptable duration; that they past-pointed to the right and to the left on an average of two or three times; and finally that they fell to the right and to the left in a pronounced manner.

III. The Organic Results of Rotation

Under the experimental conditions just described, there is produced, both during and after rotation, an elaborate array of organic and mental effects. In the naïve subject all of these effects are usually of great severity and complexity. Occasionally they become overwhelming, as in nausea.[1]

A. Local Effects: Nystagmus.—All of the organic effects have commonly been assigned, without much criticism or analysis, to two gross classes. As we have just seen, the unfailing regularity with which the ocular effects appear has earned for them a separate place. These effects, which are sometimes called 'vestibular nystagmus,' are characteristic. As rotation begins, the eye tends to lag behind the movement of the head, and then suddenly to recover its original position, only to lag behind again, and so on. After the cessation of the rotatory movement, the order of the ocular deflections is just reversed. The slow phase, which took place in the direction opposite to the original rotation, now takes place in the direction of the bodily movement. The quick phase, which previously took the direction of rotation, now runs

[1] During the course of our experiments three subjects were obliged to discontinue the series temporarily, on the first day, because of nausea. In one case only, however, did vomiting take place. Almost all the subjects were slightly nauseated on the first day but rarely on the second. As has been frequently noted, all of the organic effects are tremendously increased in intensity if the axis of position is changed at any time during rotation or during the persistence of the after-nystagmus. One subject (*O*) responded to the falling test by repeated clonic contractions of the lower limbs.

in the contrary direction. In case the head is bent forward or to the side, the movement is circular but of the same general kind. We are here concerned only with the horizontal movements aroused by rotation about an axis lying in or near the head. Furthermore, the ocular effects will make up the bulk of our experimental evidence on the nature of the organic effects of rotation and we shall be primarily concerned with the movements occurring after rotation ceases.

B. Systemic Effects: Muscular Innervations: Visceral and Vascular Changes.—The other organic effects which have come to occupy a prominent place are certain innervations of the limbs and of the body as a whole due to a diffuse involvement of the whole skeletal musculature. If the limbs or the head are held free from support, they tend to move in a characteristic manner. The head may turn to one side or to the other, depending upon the direction of bodily rotation, and the limbs on the side of the body toward the rotation tend to move in the same direction. Furthermore, the immanence of nausea, of excessive perspiration, and the 'feel' of internal changes in the viscera, the lungs and the head suggest that the excitation of the canals has a direct influence on many of the internal organs and upon the autonomic system.

Aside from these general organic effects, which we shall consider in a secondary way only, there is a large class of effects that are commonly dismissed as 'mental.' They are usually spoken of as 'vertigo,' 'dizziness' and nausea. As a matter of fact, analysis reveals a host of kinæsthetic sensations localized less clearly about the trunk and the limbs than about the neck, the face, and especially the eyes. The trained observer discovers a background of organic processes which seem finally to become localized about the lower end of the esophagus. Visceral coolness and pressure, clearer than the other processes, are occasionally accompanied by a 'bad taste' in the mouth, an excessive flow of saliva, hints of a disagreeable olfactory quality, and a diffuse 'feel' of peripheral warmth; and the culmination of the whole experience may come in the form of kinæsthetic pulls and tensions throughout the viscera and abdominal walls—the processes characteristic

of nausea and reversed peristalsis. In a subsequent investigation we shall undertake in a serious manner to analyze these psychophysical factors and to point out their relation to the whole experience.

IV. Physical and Physiological Factors which Modify the Organic Results of Rotation

We come now to the central part of our experimental enquiry. The whole course of the investigation goes to show that nystagmus is only a small part of the total organic effect of rotation[1] and that all of the effects, both local and systemic, tend to decrease in complexity and in intensity and finally to disappear under the influence of prolonged turning. That is to say, our investigation shows that these effects can be profoundly modified in their appearance by certain physical and physiological factors which we shall immediately describe.

A. Modification by Periodic Repetition.—Of the progressive changes occurring on the organic side, the most outstanding is the decrease from day to day and the decrease within a single diurnal series of the duration of the after-nystagmus, of the number and the amplitude of the ocular movements, and finally, of the time of apparent movement in the visual field, due to periodic repetition. Tables II. and III. give, for each of 16 subjects, the averages for rotation alternately to the right and to the left in the successive daily series. Figure 1 is a typical curve based on the results obtained from subject *K*.

When Tables II., and III., and Fig. 1 are carefully scrutinized the following facts appear:

(*a*) Every observer shows more or less decrease in the duration of nystagmus, in the number of ocular movements,

[1] The otologists have drawn a peculiar and curious distinction between nystagmus and the other effects of rotation, principally vertigo. For example, Jones (*op. cit.*, p. 5) says that "ear-stimulation produces certain definite phenomena—a rhythmic jerking of the eyes known as nystagmus, and a subjective sensation of turning which may be termed a systematized vertigo. The two distinct phenomena then are nystagmus and vertigo." We shall have occasion to discuss this distinction at another time. At present we must insist that the distinction is but a gross logical separation of the effects induced.

TABLE II

AVE. TIME (SECS.) OF AFTER-NYSTAGMUS FROM TEN ROTATIONS (5 TO R. AND 5 TO L.) OF TEN REVOLUTIONS EACH, FOR SIXTEEN SUBJECTS (*A–P*).

Series	Subjects																																
	A	*B*	*C*	*D*	*D* Con.	*E*	*E* Con.	*F*	*F* Con.	*F* Con.	*F* Con.	*F* Con.	*F* Con.	*G*	*G* Con.	*G* Con.	*G* Con.	*G* Con.	*H*	*H* Con.	*I*	*J*	*K*	*K* Con.	*L*	*L* Con.	*M*	*M* Con.	*N*	*N* Con.	*O*	*P*	*P* Con.	*P* Con.
1	17.9	17.6	22.9	16.4	5.1	17.2	2.9	18.5	10.9	9.1	9.0	6.9	5.3	17.6	10.2	7.3	4.7	1.6	24.5	4.2	14.4	20.0	21.5	7.3	18.2	4.1	21.7	6.9	18.7	8.7	15.9	25.5	8.7	3.7
2	18.4	8.7	22.2	13.5	6.1	14.9	3.4	18.3	9.8	9.6	9.5	6.0	4.8	16.4	10.8	7.7	5.0	1.0	19.6	4.1	15.0	14.5	21.5	5.5	16.5	3.6	18.7	8.9	18.2	6.9	7.3	27.0	7.5	2.5
3	17.2	6.4	21.9	12.3	6.7	14.7	2.6	16.4	10.4	9.9	8.1	6.9	4.1	15.5	10.0	7.7	4.3	1.0	16.6	3.7	13.8	17.7	22.4	5.6	12.0	2.9	16.4	5.8	16.7		6.0	25.2	5.5	1.0
4	14.6	6.4	23.7	14.7	6.0	12.0	2.2	13.5	9.3	9.2	8.3	6.3	4.5	14.0	9.8	8.0	5.0	0.7	13.5	3.5	12.1	12.1	17.1	5.3	15.1	3.1	15.3	6.4	15.6		3.3	22.3	6.7	0.0
5	13.0	4.8	21.9	15.5	5.6	10.5	0.7	14.8	9.4	9.4	7.6	6.4	4.0	13.5	8.9	8.8	4.0	0.2	11.8	2.6	14.0	11.8	16.2	3.6	12.5	2.8	15.5	6.3	14.8		1.9	18.3	6.5	0.0
Av.....	15.8	8.8	22.5	14.7	5.9	13.9	2.3	16.3	10.0	9.4	8.5	6.5	4.5	15.4	9.9	7.9	4.6	0.9	17.2	3.6	13.9	15.2	19.7	5.4	14.8	3.3	17.5	6.8	16.8	7.8	6.9	23.7	7.0	1.4
6	9.6	4.6	19.4	13.1	6.6	8.5	0.0	13.5	10.4	8.8	8.4	5.6		12.6	9.3	7.1	3.6	0.0	10.4	1.3	12.1	10.4	15.3	3.0	11.3	2.0	14.3	6.8	13.9		1.5	16.8	6.3	
7	8.0	2.5	18.5	12.9	5.3	8.7	0.0	12.4	10.6	9.0	7.4	6.3		10.8	8.4	7.7	2.8	0.0	8.6	0.0	12.6	11.1	14.2	3.8	11.5	3.0	18.1	9.6	13.5		1.2	16.9	5.1	
8	7.3	3.1	18.6	12.0	6.8	7.2		11.4	10.4	8.9	8.3	5.4		10.0	8.5	5.8	3.8		9.1	0.0	11.1	11.4	12.4	2.9	10.5	1.6	12.3	10.1	13.3		1.5	14.2	5.4	
9	8.8	2.3	20.4	9.4	6.3	7.6		11.3	10.0	9.7	7.3	6.1		10.7	8.5	5.7	2.8		7.5	0.0	10.7	11.8	11.3	2.6	9.2	2.1	11.3	6.1	10.3		0.1	13.2	5.3	
10	8.5		17.2	8.2	7.4	6.4		13.6	11.1	8.4	7.8	5.0		11.4	8.9	5.5	3.3		8.3		10.0	11.4	10.2	2.1	9.0	0.6	10.1	4.9	11.9		0.0	11.9	5.7	
Av.....	8.4	3.1	18.8	11.1	5.3	7.7		12.4	10.5	9.0	7.8	5.7		11.1	8.7	6.4	3.2		8.8		11.3	11.2	12.7	2.9	10.3	1.8	13.2	7.5	12.6		0.8	14.6	5.6	
11	7.3		18.2	8.4	7.5	6.5		11.5	9.5	9.1	6.5	6.1		11.1	9.1	6.4	2.8		7.1		12.5	11.0	11.1	2.4	7.9	0.0	11.1		9.4			10.5	4.7	
12	6.7		18.9	8.8	4.6	5.7		11.4	10.4	8.2	7.0	5.4		10.3	8.5	5.4	2.3		7.6		10.6	10.9	9.5	1.8	7.2	0.0	8.5		9.5			11.8	4.1	
13	6.2		17.4	9.1	5.8	6.9		10.7	10.1	10.3	6.8	5.5		11.4	8.9	5.5	2.2		6.7		9.7		9.0	1.2	6.4		11.6		9.6			11.2	4.2	
14	4.8		15.9	7.8	3.8	5.3		11.5	10.7	7.9	7.2	4.8		10.4	8.4	5.4	2.3		5.6		9.9		7.2	0.0	5.2		7.3		8.9			7.3	3.6	
15	3.8		14.6	7.0	3.0	5.3		9.8	9.7	9.8	6.2	5.1		10.1	7.9	5.4	2.0		4.7		10.2		7.3	0.0	5.3		9.1		7.8			7.3	3.5	
Av.....	5.5		17.0	8.2	4.9	5.9		11.0	10.1	9.1	6.7	5.3		10.6	8.4	5.6	2.3		6.3		10.6	10.9	8.8	1.1	6.4		9.5		9.0			9.6	4.0	
Decrease (sec.).....	14.1	15.3	8.3		13.4		17.2						14.5					17.6		24.5	4.2	9.1		21.5		18.2		16.8		11.8	15.9			25.5
Decrease (%)	80	87	36		81		100						84					100		100	29	45		100		100		77		63	100			100
Rate of decrease per trial.....	.94	1.70	.55		.45		.82						.18					.27		1.11	.28	.75		.77		.73		.67		.70	1.59			.78

TABLE III

AVE. NO. OF OCULAR MOVEMENTS FROM TEN ROTATIONS (5 TO R. AND 5 TO L.) OF TEN REVOLUTIONS EACH, FOR 16 SUBJECTS (*A–P*).

Series	Subjects																															
	A	*B*	*C*	*D*	*D* Con.	*E*	*E* Con.	*F*	*F* Con.	*F* Con.	*F* Con.	*F* Con.	*F* Con.	*G*	*G* Con.	*G* Con.	*G* Con.	*G* Con.	*H*	*H* Con.	*I*	*J*	*K*	*K* Con.	*L*	*L* Con.	*M*	*M* Con.	*N*	*N* Con.	*O*	*P*	*P* Con.	*P* Con.
1	15.5	10.9	36.5	8.2	3.8	20.2	2.9	39.0	25.0	13.6	11.2	9.5	6.9	34.5	17.7	10.9	5.2	1.8	56.1	5.3	20.8	47.9	32.5	10.6	26.5	5.5	35.2	9.7	35.0	13.6	10.8	47.5	7.0	2.6
2	20.3	4.3	40.5	5.5	5.0	22.6	3.1	39.5	20.3	12.6	13.6	6.7	5.2	31.2	18.6	10.9	5.2	1.2	51.3	4.3	25.4	26.8	30.4	7.2	28.0	5.2	30.7	11.0	30.2	9.9	8.1	37.0	5.4	2.0
3	18.0	2.3	38.8	5.1	5.5	16.3	2.7	36.3	22.8	16.6	10.5	9.1	4.8	31 0	17.3	11.0	4.3	1.2	38.7	4.1	20.6	39.9	40.4	6.2	18.6	4.8	24.9	7.2	35.2		5.8	40.2	4.6	0.5
4	14.1	2.6	39.9	5.9	4.7	13.0	1.9	30.7	15.7	14.2	12.3	7.6	6.0	26.6	18.3	11.7	5.2	0.7	31.2	3.7	24.6	25.3	30.5	8.1	28.4	4.7	25.6	8.0	27.5		4.0	35.8	5.7	0.0
5	11.9	4.0	40.8	7.2	4.7	11.0	0.7	33.1	17.8	16.4	10.7	8.2	4.0	26.9	14.5	11.7	3.8	0.2	27.2	2.8	25.6	29.1	29.5	4.4	20.6	3.7	25.9	7.4	25.3		2.0	27.0	6.2	0.0
Av.	15.9	4.8	39.3	6.4	4.7	16.7	2.2	35.7	20.3	14.7	11.7	8.2	5.4	30.0	17.3	11.2	4.7	1.0	40.9	4.0	23.4	33.8	32.6	7.3	24.4	4.8	28.4	8.6	30.6		6.1	37.5	5.8	1.0
6	7.9	2.6	34.9	5.2	5.8	9.5	0.0	30.6	18.7	13.3	13.8	6.6		25.0	15.2	11.1	3.7	0.0	22.9	1.5	26.6	24.6	24.0	3.3	21.0	2.9	23.7	5.3	24.8		1.7	25.2	5.2	
7	5.0	2.1	34.7	6.3	4.6	9.9	0.0	27.5	15.4	14.8	9.8	8.0		19.9	12.9	10.2	2.8	0.0	18.5	0.0	25.2	22.3	20.0	5.1	19.1	3.6	28.1	11.0	20.8		1.6	20.5	4.5	
8	4.5	3.3	37.4	6.1	5.9	7.9		24.5	19.3	10.6	13.4	7.3		17.4	13.1	7.5	4.0		16.6	0.0	21.8	26.1	20.9	3.3	19.0	1.9	17.3	11.2	20.7		1.4	19.1	4.7	
9	9.9	2.8	37.4	4.5	4.6	7.7		27.2	15.0	14.3	9.3	8.2		18.1	11.9	7.2	3.0		11.4	0.0	22.0	30.7	19.1	2.9	16.9	2.5	14.5	6.9	17.7		0.1	18.2	4.7	
10	8.4		30.5	3.8	5.9	5.2		32.1	18.9	11.1	12.8	7.2		20.0	13.4	7.8	3.3		12.1		18.4	27.4	17.2	2.0	15.4	0.5	13.7	5.2	16.1		0.0	16.3	4.4	
Av.	7.1	2.7	35.0	5.2	5.7	8.0		28.4	17.5	12.8	11.8	7.4		20.1	13.3	8.7	3.4		16.3		22.8	26.0	20.2	3.3	18.3	2.3	19.4	7.9	20.0		1.0	19.8	4.7	
11	8.2		33.3	3.5	6.9	6.5		26.7	15.2	18.3	8.3	7.9		18.1	13.3	7.4	2.9		12.0		25.0	26.2	15.5	2.4	12.3	0.0	15.7		14.7		0.0	12.6	4.0	
12	7.9		33.4	3.7	3.1	5.1		28.6	16.8	12.9	11.3	6.4		17.5	11.4	6.3	2.5		10.6		18.4	22.3	14.0	1.8	11.5	0.0	11.6		13.7			15.5	3.6	
13	5.1		32.4	3.6	5.1	6.1		24.9	17.2	18.0	8.9	6.2		19.2	12.8	6.5	2.3		9.6		16.0		13.1	1.3	10.6		18.0		13.4			10.9	3.2	
14	4.9		30.4	6.1	2.6	4.2		29.8	17.9	11.0	11.1	6.2		17.9	12.0	5.4	2.2		7.0		18.0		9.1	0.0	7.7		11.4		12.2			5.2	3.3	
15	3.6		29.5	3.5	1.9	4.8		20.4	14.2	15.1	7.9	6.8		17.1	11.6	6.2	1.9		6.2		15.2		9.2	0.0	7.8		11.6		11.4			5.5	2.6	
Av.	5.9		31.8	4.1	3.9	5.4		26.1	16.3	15.1	9.5	6.7		18.0	12.2	6.4	2.4		9.0		18.5		12.2	1.1	10.0		13.6		13.1			9.9	3.3	
Decrease (No.)	11.9	8.1	7.0		6.3		20.2						35.0					34.5		56.1	5.6	25.6		32.5		26.5		30.0		25.1	10.8			47.5
Decrease (%)	76	74	19		77		100						90					100		100	27	53		100		100		85		72	100			100
Rate of decrease per trial	.79	.90	.47		.21		.92						.37					.51		2.55	.37	2.13		1.17		1.06		1.20		1.48	1.20			1.40

and in the time of apparent movement in the visual field.[1] It will be observed that the three representative values decrease together. The decrease in duration of nystagmus varies from 29 per cent. in the case of subject *I*, to 100 per cent. for subjects *E*, *G*, *H*, *K*, *L*, *O* and *P*. The average amount of decrease for all subjects in these tables turns out to be 79 per cent. or more than three fourths of the initial time. Furthermore, there is every reason to believe that had the several series been continued sufficiently, all subjects would have decreased 100 per cent. The decrease in the number of movements varies from 19 per cent. for subject *C* to 100 per cent. for subjects *E*, *G*, *H*, *K*, *L*, *O* and *P*; again, an average of 79 per cent. The same ratio holds true also for the duration of the apparent movements in the visual field.

FIG. 1.

(*b*) There is a wide individual difference in the initial values. Subject *I* gave an average nystagmus time for the first ten trials of 14.4 sec. while subject *P* gave 25.5 sec. It will be observed that most of these initial values are below those given by the otologists as constituting 'normality.' It will be recalled that this 'normality' value was about 25 sec. As a matter of fact, the average of all our subjects for the first ten trials is but 18.1 sec. This one comparison is an outstanding example of the sensitivity of the ocular responses to repetition. The low average found in our case is due to the fact that our initial values are an average of the first *ten* rotation periods of ten revolutions each. Within these ten

[1] As can be seen from Fig. 1, the line showing the time of apparent movements almost coincides with the line showing the time of nystagmus. That is to say, a table similar to Tables II. and III., but giving these times of apparent movement, would almost duplicate Table II., and we have not, therefore, included such a table.

TABLE IV

SHOWING, FOR EACH OF 16 SUBJS., TIME OF AFTER-NYSTAGMUS, NO. OF OCULAR MOVEMENTS, AND TIME OF APPARENT MOVEMENT IN THE VISUAL FIELD FOR (*a*) THE FIRST TRIAL TO THE RIGHT AND TO THE LEFT, (*b*) THE AVERAGE OF THE FIRST TEN TRIALS (5 R. AND 5 L.), AND (*c*) THE LAST TRIAL TO THE RIGHT AND TO THE LEFT. A TRIAL CONSISTS OF TEN REVOLUTIONS.

		Subjects																
		A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*	*L*	*M*	*N*	*O*	*P*	Ave.
Time of After-nystagmus (secs.)	First Trial R.	17.0	25.0	31.0	18.0	16.0	18.0	19.0	31.0	22.0	23.0	20.0	22.0	21.0	19.0	26.0	32.0	22.5
	First Trial L.	22.0	20.0	23.0	24.0	20.0	27.0	17.0	25.0	23.0	29.0	24.0	20.0	26.0	22.0	22.0	30.0	23.4
	Av. 1st 10 Trials	17.9	17.6	22.9	16.4	17.2	18.5	17.6	24.5	15.4	20.0	21.5	18.2	21.7	18.7	15.9	25.5	18.1
	Tenth Trial R.	15.0	14.0	22.0	11.0	12.0	16.0	18.0	24.0	15.0	17.0	20.0	13.0	18.0	16.0	11.0	21.0	16.4
	Tenth Trial L.	17.0	14.0	19.0	24.0	16.0	16.0	16.0	20.0	13.0	18.0	20.0	18.0	14.0	17.0	8.0	21.0	16.8
No. of Ocular Movements	First Trial R.	13.0	15.0	55.0	10.0	20.0	38.0	39.0	58.0	25.0	53.0	44.0	37.0	36.0	38.0	20.0	50.0	34.4
	First Trial L.	24.0	12.0	42.0	12.0	21.0	46.0	35.0	50.0	23.0	71.0	41.0	25.0	44.0	31.0	18.0	52.0	34.2
	Av. 1st 10 Trials	15.5	10.9	36.5	8.2	20.2	39.0	34.5	56.1	20.8	47.9	37.5	26.5	35.2	35.0	10.8	47.5	30.1
	Tenth Trial R.	11.0	9.0	32.0	5.0	17.0	38.0	33.0	61.0	21.0	37.0	33.0	21.0	28.0	29.0	7.0	50.0	27.0
	Tenth Trial L.	15.0	8.0	30.0	10.0	18.0	35.0	25.0	51.0	18.0	45.0	31.0	27.0	24.0	29.0	7.0	38.0	25.6
Time of Apparent Movement (secs.)	First Trial R.	17.0	20.0		17.0	16.0	20.0	19.0	30.0	22.0	25.0	20.0	23.0	20.0	19.0	26.0	32.0	21.7
	First Trial L.	20.0	18.0		24.0	20.0	20.0	18.0	25.0	21.0	30.0	24.0	20.0	22.0	18.0	22.0	30.0	22.1
	Av. 1st 10 Trials	16.7	15.9		17.1	18.0	18.6	17.1	24.1	11.7	19.6	21.7	16.7	19.7	17.7	15.7	26.5	18.4
	Tenth Trial R.	15.0	14.0		12.0	14.0	16.0	18.0	24.0	13.0	16.0	20.0	9.0	17.0	16.0	11.0	24.0	16.7
	Tenth Trial L.	15.0	11.0		22.0	19.0	16.0	13.0	20.0	9.0	16.0	20.0	10.0	13.0	17.0	8.0	21.0	16.0

rotation periods the decrease is already so great as appreciably to lower the average time of nystagmus.

By referring to Table IV. we get a more detailed analysis of the first ten trials for our sixteen subjects. It will be seen from this table that the average initial nystagmus times for these subjects were R. 22.5 sec., and L. 23.4 sec., values which fall close to the median assumed by otologists to be "normal." [1] The striking fact, however, is the decrease within the first few minutes of rotation, a decrease which is apparently due to the cumulative effect of repetition.

(*c*) The decrease in all the values is of a characteristic kind. We have just pointed out the marked decrease within the first ten trials. Reference to any of the preceding tables or to the figure will show that a large part of the decrease takes place in the first few days. That is to say, there is a sudden drop in the curves which represent the temporal course of the nystagmus from day to day. We have found that all the subjects showed an average decrease of 79 per cent. in the time of nystagmus. But an average decrease of 57 per cent. occurs during the first half of the series, leaving the remaining 22 per cent. to be distributed over the last half of a series. Table V. shows, in percentages, this decrease

TABLE V

Subjects	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*	*L*	*M*	*N*	*O*	*P*	Av.
% decrease first half.....	55	72	19	54	63	50	56	71	35	50	66	60	63	41	90	70	57

[1] In view of the otological practice, these standard or 'normal' times should not be taken too seriously. In one list of about 1800 'official' examinations the duration of the after-nystagmus actually observed (omitting the extreme times) ran as follows (after one turn of 10 revolutions to the right):

Duration.....	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31 sec.
No. examined ..	32	70	41	137	24	*239*	49	137	102	137	146	158	54	66	27	98	10

The greatest number of cases for 'normal' men seems, then, to fall, not at 25 sec. or 26 sec., as we should expect, but at 20 sec. The table shows a striking distribution about 20 sec. and 30 sec. Possibly the otologist's watch has a preference for round numbers. If it has, it yields in a fair and openminded way at 26, the exact mean in the 'allowed' normal range of 16—36, and also at 16, the lowest permissible 'normal' for aviators. The sudden drop on either side of 16 sec. would seem to emphasize the high clinical importance of that number; while the small number of 15-sec. cases would seem to suggest that Nature does her best, in a great human emergency, to follow the prescriptions and to exemplify the discoveries of her learned children.

during the first half of all the series of the sixteen subjects. These figures are more significant than at first appears. Only a few of the series were carried to completion, *i.e.*, to zero. On this account, the distribution of the amount of decrease cannot be fully represented. In those cases where the series did decline to zero the average decrease for the first half of the trials was 70 per cent.

Another indication of the difference in the rate at which decrease occurs early and late in a series is found in the size of the corresponding mean variations. Whereas the m.v. usually runs 1.5–2.0 early in a series, it hardly ever rises above 0.5 late in a series. We have already pointed out that the m.v. depends primarily upon a constant decrease in the measures averaged and not upon irregularities due to the method of observation.

(*d*) The rate of decrease is, again, a matter of individual differences. Subj. *B* decreased at the rate of 1.7 sec. per trial, while Subj. *F* decreased at the much slower rate of .2 sec. per trial. The same relative values hold also for the number of movements and for the duration of the apparent movement. Here again, the rate of decrease is greater during early trials than later in the series. In the cases just mentioned, the rate of decrease for the first ten series in Subj. *B* was 2.5 sec. per trial and in Subj. *F* it was .7 sec. per trial. For the last ten series, Subj. *B* showed a rate of .5 sec. and Subj. *F* but .01 sec. per trial.

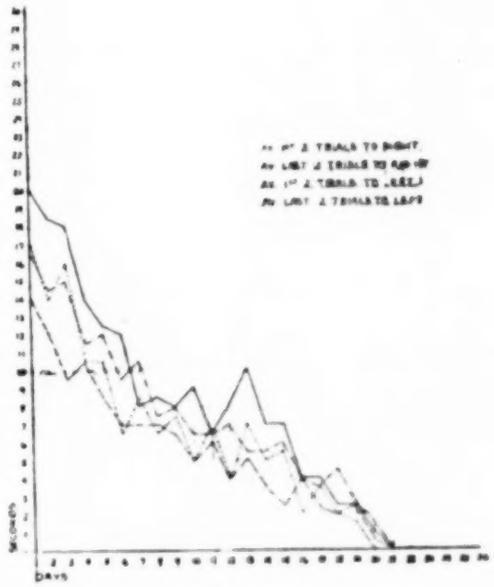

FIG. 2

Other averages testify to the decrease of all values from day to day. (1) If the first trials of each day are thrown together, it will be found that the first trial of any single day is not so large as the first trial of the day preceding, although

it may be a little larger than the *last* trial of the preceding day. That is, there seems to be a slight tendency to revert to the original values because of the lapse of the 24 hours. This factor will be studied in detail below. The decrease from day to day as shown by all first and last turns of rotation to the right and the same for rotation to the left is presented in Fig. 2 (Subj. *E*). This again is a typical curve. It exemplifies the various facts and regularities of decrease from day to day.

(2) There is, however, a still more striking statement of the sensitiveness of nystagmus to practice than we have yet given. So far we have mainly considered decreases from day to day. Table VI. shows decreases that also take place within a single day. Here again the decrease is of the same kind as that already described. It will be noted that the total decrease during the first five days is notably greater than during the last five days.

TABLE VI

SUBJECT *E*

Amount of decrease in time of nystagmus within single rotation-periods of ten trials each. Averages of the first two trials to the right and to the left on the first five days are compared with the averages of the last two trials to the right and to the left during the same five days and so on for succeeding five-day periods.

		Right			Left		
		T.	No.	A.M.	T.	No.	A.M.
1st 5 days...	Av. 1st 2 trials.....	16.6	21.6	14.8	13.9	16.3	14.5
	Av. last 2 trials.....	13.6	16.5	13.2	10.9	12.8	10.1
	Decrease.....	3.0	5.1	1.6	3.0	3.5	4.4
2d 5 days....	Av. 1st 2 trials.....	9.8	11.5	8.7	9.5	9.5	10.2
	Av. last 2 trials.....	7.9	8.9	7.4	7.2	7.3	7.7
	Decrease.....	1.9	2.6	1.3	2.3	2.2	2.5
3d 5 days ...	Av. 1st 2 trials.....	8.1	7.6	8.7	6.1	5.4	6.8
	Av. last 2 trials.....	5.6	4.9	5.7	4.9	3.6	5.0
	Decrease.....	2.5	2.7	3.0	1.2	1.8	1.8
4th 5 days...	Av. 1st 2 trials.....	4.0	4.0	4.7	4.1	3.7	4.7
	Av. last 2 trials.....	2.9	2.6	3.4	2.3	2.2	2.9
	Decrease.....	1.1	1.4	1.3	1.8	1.5	1.8

(3) Again, the decrease of the ocular effects from day to day is demonstrated as follows: The *number* of movements *per second* tends, in most subjects, to decrease along with the more apparent decrease of time-values. This fact is a measure of the decrease in the intensity of the ocular effects, while the preceding facts have been concerned only with the *duration* of the movements and with their *total number*. For example, Subj. *E* decreased from an average of 1.18 movements per second in the first five periods to 0.96 in the last five periods. Subj. *F* decreased from 2.20 to 1.35; Subj. *G* from 1.95 to 1.10; Subj. *J* from 2.76 to 2.27; Subj. *I* from 1.49 to 1.33; Subj. *H* from 2.35 to 1.17. Two subjects showed a slight apparent increase. It was found, however, that these two subjects, who had very wide initial movements, fell into the habit of gazing fixedly in the field of vision, a procedure which decreases the *amplitude* of the eye movements but increases the *speed* and probably the *time* of nystagmus.

(4) And, finally, we discover that the *amplitude* of the eye-movements becomes less and less as a series proceeds. Readings of the after-nystagmus taken through a reading microscope show that the eye movements in some observers decrease from an amplitude of 8 mm. to an amplitude of less than 0.5 mm.[1] For example, in the case of Subj. *O* the amplitude of the movements took the following course:

TABLE VII

Days	Ampl.	Days	Ampl.
1	6.0 mm.	6	1.6 mm.
2	4.0 "	7	1.2 "
3	3.5 "	8	0.8 "
4	2.4 "	9	0.5 "
5	1.9 "	10	0.0 "

So far we have been concerned wholly with the ocular effects of rotation. It has been pointed out, however, that these effects are but a portion of the whole organic group. Now all of the other effects follow the same course as the

[1] See Bárány, R., 'Apparat zur Messung der Rollbewegungen des Auges,' *Zeitschr. f. Sinnesphysiol.*, 1911, 45, 59–62, for a description of an apparatus made and used by him for measuring certain eye-movements.

ocular resultants. Most of the subjects were more or less nauseated during the first trials. Nevertheless, within a day or two, all nausea disappeared for most of the subjects and within three or four days no trace of it was left. The same is true of the tendency to excessive perspiration and of the tendency to hold the breath during rotation.

We have already observed that one of the principal tests for vestibular 'normality' given by the otologists is the test called 'past-pointing.' It was found in the case of naïve observers that they tended after rotation to point past,—either to the right or to the left of,—any object which had been previously designated. We have found, however, that, after practice, subjects no longer tend to 'past-point' in this way. That is to say, it is possible, by measuring the amount of past-pointing, to demonstrate that, as practice continues, the angle of deflection of the hand and arm becomes constantly lessened; so that, finally, there is no deviation at all. A quantitative experiment was performed in the following manner. The subject was instructed to hold his arm loosely in front, a pencil held lightly in his fingers, resting upon a large sheet of paper. He was further instructed to let the arm move as it would and if it should at any time move off the paper, to return as nearly as possible to the starting place and to let the arm again move involuntarily. Under these conditions, it was found that, during early rotational periods, either arm tended to move out and away from the body, no matter what the direction of rotation. During the stopping period, the arm moved in and toward the body for a moment and then resumed its original direction with rapidly decreasing deflection. Finally it was found that these movements tended to drop out. Fig. 3, made up from the actual records, will schematize the facts concerning these movements.

It will be seen from Fig. 3 that the movement of the right arm to the right during rotation to the right must be caused by a definite innervation of the muscles of that arm. On the other hand, the movement of the right arm to the right during rotation to the left suggests that the inertia of the arm is the principal factor. The same is true of the left arm.

The movement of the left arm to the left during rotation to the left seems to be the result of definite innervation, whereas the movement of the left arm to the left during rotation to the right suggests mere inertia. Introspective reports of the difference in the clearness and intensity of the muscular

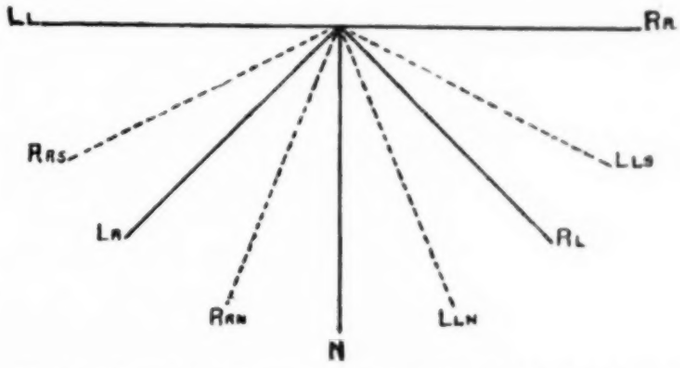

FIG. 3. *N*, the direction of movements made with either the right or the left hand while the body is at rest. *Rr*, direction of movements made by the right hand during rotation to the right. *Ll*, direction of movements made by the left hand during rotation to the left. *Rl*, direction of movements made by the right hand during rotation to the left. *Lr*, direction of movements made by the left hand during rotation to the right. *Lls*, direction of movements made by the left hand just as rotation to the left is ceasing. The direction then changes to *Lln* and finally to *N*. *Rrs*, the direction of movements made by the right hand just as rotation to the right is ceasing. The movements then take the direction of *Rrn* and finally of *N*. As practice continues, *Ll* and *Rr* drop down to *Lr* and *Rl*, then to *Rrn* and *Lln* and finally take the direction of *N* as repetitions increase.

sensations set up during this time bear out the fact that the one is the result of definite innervation while the other is expressive of inertia. The lower limbs tend to behave in this same general manner, *i.e.*, the right leg during rotation in either direction tends to swing to the right while the left leg during rotation in either direction tends to swing to the left.

B. Physical Conditions.—Thus far we have found the appearance of nystagmus to be inversely proportional to the number of turnings. That is to say, the time of nystagmus, the time of apparent movement of objects, and the number of eye-movements decrease very materially from day to day as rotation is continued. So sensitive is nystagmus to practice of this kind that an appreciable decrease in the above values occurs within a single period of ten trials. We have also pointed out certain peculiarities of this decrease. Fur-

thermore, we have shown that the ocular movements decrease in number and amplitude, a fact which is paralleled by the decrease and final disappearance of the other organic effects, such as past-pointing. In other words, nystagmus is not a fixed response to rotation: it is clearly affected by repetition. We now proceed to still further evidences of the modifiability of the ocular responses.

1. *Speed of Turning.*—It has already been suggested that the time of nystagmus is directly proportional to the number and to the speed of turnings. We have found that a single rotation will rarely, if ever, result in any observable effects. After two revolutions, there is usually a brief flicker of the eyes. From this number up to the full number ten, the effects increase rapidly in duration and in intensity. Even after much practice any change in the rate of rotation or in the number of revolutions will change the appearance of the nystagmus. For example, near the end of their respective series, subjects *E* and *H* were rotated twice as fast as in the regular series, *i.e.*, ten times in ten seconds. The values from the regular series at this time for Subj. *E* were 3.4, 3.1, 4.4 and for Subj. *H* 4.2, 5.3, 4.4. After the trials at increased speed the corresponding values for *E* were 14.5, 17.5, 14.0 and for *H* 17.5, 35.0, 15.5. The very large increase under the new conditions is evidence enough that the speed of rotation seriously affects the appearance of the nystagmus even though much practice has already taken place. It is significant, however, that the larger values are but a fraction of the values obtained at a similar speed before any practice.

2. *The Mode of Stopping.*—Subj. *C* was stopped slowly and abruptly on alternate turnings, care being taken that no advantage fell in either direction. The average nystagmus for all slow stoppings was 16.9 scc. and the number of movements was 30.9. For abrupt stoppings the averages were 21.7 and 40.6. That is to say, nystagmus was increased both in duration and in intensity when a subject was abruptly stopped.

3. *The Effect of a Brief Reversal of the Direction of Rotation.*—Returned aviators reported that frequently during the official test given by the otologists the rotating chair had

been allowed to swing past the correct stopping place and had then been brought back to position before readings were taken. Table VIII. shows the results obtained from Subj. *N* by allowing him in alternate trials to move one quarter turn too far in the direction of rotation and then to bring him back to the usual stopping place. The additional time required was one second. It is clear that such a brief return movement serves considerably to decrease the time of nystagmus and the number of eye-movements.

TABLE VIII

Trials	Normal Stop			Excess and Return		
	T.	No.	A.M.	T.	No.	A.M.
First 50	17.5	33.5	17.4	15.7	26.8	15.4
Second 50	14.2	25.7	14.2	10.9	14.4	10.9
Third 50	10.5	16.7	10.7	7.4	9.5	7.5
Final 20	9.3	14.9	9.2	6.3	8.6	6.2

C. Temporal Conditions.—The organic effects of rotation are also modified by temporal conditions. These are of two kinds, *viz.*, the time of day at which rotation takes place, and the interval that elapses between successive rotation periods.

TABLE IX

Five-day Periods	Time	No. of Mvts.	Time of App. Mvt.
1	1.1	2.2	1.1
2	1.3	6.9	1.3
3	0.5	3.5	0.4
Total	2.9	12.6	2.8
4	0.9	3.7	0.9
5	1.7	4.5	1.3
6	1.1	3.0	1.2
Total	3.7	11.2	3.4

1. *The Time of Day.*—Subj. *F* was rotated regularly every morning at 7:30 and every evening at 6:30. Both periods came just after meals. Efforts were made to keep other factors constant. The averages for the morning turnings (32 in all) were 10.4, 19.6, 10.8 and those for the evening

turnings (31 in all) were 9.5, 16.1, 9.9. That is, the values for the evening trials were a little smaller than for the morning trials. That this difference is more significant than at first appears, may be seen by referring to Table IX. The figures in this table represent the average amount by which the evening turnings were less than morning turnings in terms of five-day periods.

2. *The Effect of Intervals.*—(*a*) The effect of short rest intervals between trials. Subjects *E* and *H* were alternately rotated with no rest between successive turnings and with rest (2 min. between R. and L. and three min. before another pair to R. and L.) between successive turnings. Subj. *E* shows that the decrease is faster (measured here by smaller averages) when no rest is allowed between turnings (Table X.). Subj. *H* yields no positive evidence for the effect of

TABLE X

	E			*H*		
	T.	No.	A.M.	T.	No.	A.M.
No Rest						
Av. first 50 trials	9.8	11.6	9.2	13.8	29.4	11.6
Av. second 50 trials	3.5	3.2	3.8	4.6	6.2	4.5
Rest						
Av. first 50 trials	11.7	13.1	11.6	13.1	27.7	12.0
Av. second 50 trials	4.9	4.5	5.4	5.6	5.9	4.7

intervals between trials. In other words, we have a further suggestion that the reduction in the time of nystagmus is dependent not upon fatigue but upon some factors at present unknown.

(*b*) The effect of long rest intervals between series. It was impossible to avoid missing a day or two now and then as the various series went on. We have taken advantage of this fact in Table XI. by counting the number of decreases and increases in the values after no interval and comparing these sums with the decreases and increases falling after a short interval in the series of one or two days. By inspecting the Table (XI.) it will be clear that an interval occurring in a series does not greatly alter the course of the nystagmus. Although there are relatively more increases than there are

decreases after an interval than is the case where no interval is involved, yet the difference is not significant. All that can possibly be demonstrated is that the slight diurnal loss of improvement mentioned on p. 34 is a little more evident. On the other hand, we can legitimately cite these facts as further evidence that fatigue does not play a part in the decrease of nystagmus during practice.

TABLE XI

EFFECT OF A SMALL INTERVAL OF NO-PRACTICE ON THE COURSE OF THE NYSTAGMUS. THE RELATIVE NUMBER OF DECREASES AND INCREASES IN THE TIME OF NYSTAGMUS WHERE NO INTERVAL HAS OCCURRED IS TO BE COMPARED WITH SIMILAR VALUES WHERE AN INTERVAL HAS OCCURRED

Subject	No Interval						Interval					
	Decreases			Increases			Decreases			Increases		
	T.	No.	A.M.	T.	No.	A.M.	T.	No.	A.M.	T.	No.	A.M.
A.....	9	9	8	1	1	2	3	3	3	1	1	1
B.....	5	4	4	1	3	3	1	1	0	0	0	1
D.....	15	13	15	10	11	9	2	2	2	2	2	2
E.....	10	10	11	4	5	3	4	4	4	1	1	2
F.....	33	30	28	22	25	27	0	0	0	5	5	5
G.....	32	32	33	19	18	18	7	8	7	3	3	4
H.....	13	15	15	3	1	1	5	5	4	0	0	1
N.....	10	11	10	3	2	3	2	3	3	1	0	0
P.....	15	17	15	8	5	8	5	4	5	3	3	3
Q.....	14	12	—	6	9	—	3	3	—	5	5	—
R.....	9	9	—	9	10	—	4	1	—	1	4	—
S.....	11	10	—	3	4	—	4	3	—	1	2	—
T.....	9	9	—	6	7	—	1	3	—	2	0	—
W.....	26	28	27	16	16	15	4	6	6	6	6	6
D.....	22	25	20	8	6	10	2	2	2	5	5	5
Total....	233	234	186*	119	123	99*	47	48	36*	36	37	30*

* These values are small because subjects *Q*, *R*, *S*, and *T*, were rotated with lenses and it was, accordingly, impossible to observe the apparent movement of the visual field. Subjects *C*, *I*, *J*, *K*, *L* and *M* were rotated every other day or every third day and are not, therefore, included in this table.

Still more striking is the persistence of the effects of practice over long intervals of time. We have shown elsewhere[1] that after from four to eight weeks, the effects of practice are still in evidence. That is to say, a subject whose nystagmus time has decreased to 5–6 secs., may some weeks later give, at most, only 8–10 secs. of ocular movement.

[1] Paper read before the Illinois State Academy of Sciences, Feburary, 1920. To be published in the *Proceedings* of the Academy.

For example, by referring to Tables II., and III. it will be seen that the last values for Subj. *F* in the regular series were 4.0, 4.0, 4.3. Two months later the averages were 7.5, 9.7, 8.2, and within ten trials (5 to the L. and 5 to the R.) had decreased to 5.5, 7.5, 5.5. In a similar way, Subj. *P*, who had lost all ocular movement with practice, gave after five months but three seconds of after-nystagmus. Further evidence was gained from five men of long experience in the aviation service. It was found in such cases that, although the first turnings might yield high numbers,[1] but a little practice was required to reduce all the values. For example, Subj. *AA*, who had been flying for eighteen months, was tested seven months after his discharge. Table XII. shows the results.

TABLE XII

Series	Right			Left		
	Time	No.	App. Mvt.	Time	No.	App. Mvt.
1	21.0	40.0	21.0	17.0	25.0	17.0
2	18.0	29.0	18.0	15.0	22.0	15.0
3	14.0	18.0	14.0	11.0	12.0	11.0
4	11.0	12.0	11.0	8.0	10.0	8.0

It is clear, therefore, that although the initial nystagmus times and other temporal values may approximate results obtained before practice in flying, the influence of practice is rapidly re-established. Furthermore, none of these subjects past-pointed or gave other report of organic disturbance.

D. The Effect of the General Organic State.—Some twenty times during the series of investigations subjects came to the laboratory suffering from organic disturbances in no way connected with the experiments themselves. Occasionally a subject came in from violent exercise or from exertion on excessively warm days. In all these cases, the time of nystagmus, the number of eye-movements, and the time of the apparent movement of the visual field were appreciably

[1] The high initial values of men coming from long practice in flying is not contradictory to our results. They, in common with whirling dancers, show a smaller nystagmus time only when turned under the conditions that have obtained during practice. That is to say, the effect of repetition is immediately apparent under one set of conditions only. Nevertheless, as we shall presently see, there is a rapid "transfer effect."

increased. For example, the averages of all the trials for these subjects just the day before the organic disturbances were 17.4, 28.7, 16.6. The averages of the trials taken while the subjects were thus organically disturbed were 20.1, 30.5, 19.1. The average of the first trials after recovery (recovery being assumed from the subject's own report) were 16.4, 27.2, 15.7. From these figures, the effect of the organic disturbance is apparent. Furthermore, the figures show that the effect is only temporary and that, in fact, it conceals a real decrease. In almost every case the results obtained after recovery were found to be about the same as the temporal course of the nystagmus at that time would indicate should have been the case. In addition to these facts, certain individuals reported on coming to the laboratory that they were feeling unusually fit. In these cases, the average of the trials on the preceding day were 18.7, 42.0, 18.5, while the averages on the day in question were 12.8, 28.4, 11.6, and the averages on the following day were 15.4, 31.3, 15.1.

For example, on July 7, 1919, Subj. *M* arose very early, worked hard all morning and was rotated just after returning from a rapid walk on a hot day. The preceding day his average values were 14.3, 23.7, and 12.5. On July 7, the values rose to 18.1, 28.2, and 18.2, while on July 8, the values dropped to 12.3, 17.3, and 12.3. This typical case shows that the increase was not a permanent matter, but that it merely covered a real decrease. On August 1 the average values for this Subj. (*M*) were 6.8, 5.3, and 6.7. On August 2 and 3 when *M* was suffering from a digestive disturbance the average values rose to 9.8, 11.1, and 10.0. But on the next period, the numbers declined again to 6.1, 6.9, and 6.5. We have already pointed out that, in the case of white rats, organic disturbances may have a pronounced effect upon the time of the nystagmus and upon the rate at which decrease takes place.[1] The diurnal differences noted just above may rest on conditions of the same order. All of these facts suggest the wide organic implications of nystagmus and of the condi-

[1] Griffith, C. R., 'The Decrease of After-nystagmus During Repeated Rotation,' *The Laryngoscope*, 1920, 30, 135.

tions of its decrease. These implications must be considered in any explanation of nystagmus and of its susceptibility to practice.

TABLE XIII

DECREASE WITH LENSES IS PARALLELED BY DECREASE WITHOUT LENSES: DECREASE WITHOUT LENSES IS PARALLELED BY DECREASE WITH LENSES

Subject	Date	Lenses		No Lenses	
		Time	No.	Time	No.
Q	May 14	45.9	53.6	29.0	57.0
	July 1	32.6	35.9	14.5	18.5
	July 8	19.9	24.7	9.5	13.0
	July 15	21.3	27.3	7.0	6.5
	July 16	20.8	26.4	7.5	8.0
	July 31	16.1	22.5	5.5	5.0
R	May 14	35.9	38.6	25.5	25.0
	May 27	26.6	17.1	18.0	12.5
	June 2	25.5	25.5	14.0	13.0
	June 5	16.2	10.7	7.5	7.0
	June 10	20.2	16.8	8.0	5.5
	June 14	12.0	7.2	6.0	4.5
S	May 13	33.0	53.7	25.0	40.0
	May 27	26.2	43.8	18.0	33.5
	June 3	20.7	32.9	13.0	22.5
	June 11	17.4	27.8	10.0	17.0
	June 13	15.3	22.7	7.0	13.0
T	May 14	50.0	88.5	22.5	62.5
	May 16	39.1	73.7	20.5	52.5
	May 21	30.1	60.9	15.0	34.0
	May 24	25.5	52.0	12.5	24.5
	May 26	18.7	38.1	9.5	20.0
P	May 12	44.5	29.5	25.5	47.5
	July 10	16.0	6.0	11.8	15.5
	July 14	11.0	20.5	7.3	5.5
	July 30	15.2	9.0	4.7	4.0
	Aug. 1	11.0	10.0	4.2	3.2
O	July 12	37.0	29.0	15.9	10.8
	July 19	15.0	9.5	1.9	2.0
	July 28	9.0	5.0	0.1	0.1
N	June 30	32.0	49.0	18.7	35.0
	July 10	27.5	39.5	10.3	22.8
	July 17	15.5	27.5	7.8	11.4
	July 19	13.5	24.0	6.9	9.9
H	May 16	31.5	67.5	24.5	56.1
	May 30	21.0	44.0	7.1	12.0
	June 12	11.0	5.5	1.3	1.5
	June 14	8.5	5.0	0.0	0.0

E. Modification by 'Transfer.' — We saw above (p. 42)

that aviators of long flying experience may have large initial values but that the effects of practice are rapidly restored. We then suggested that the high initial values were due to a change of the conditions. By inspecting Table XIII. it becomes clear that if sample trials making use of one set of conditions, *e.g.*, rotation without the use of lenses before the eyes, are taken during a series making use of another set, *e.g.*, rotation with lenses before the eyes, there actually is a 'transfer effect' from the one set of conditions to the other. That is to say, the decrease in the time of nystagmus without the use of lenses is paralleled by a decrease in the time if lenses are used at any part of the series. It has been contended by the otologists, for example, that any 'apparent' decrease in the duration of nystagmus may be offset by the use of lenses. Our results show that this cannot be the case inasmuch as a corresponding decrease has occurred when lenses are used temporarily, a modification which is equivalent to the decrease that would have occurred during a constant use of lenses. This modification by 'transfer' may be seen to advantage in the following facts. Subj. *P*, whose initial values were 25.5, 47.5, and 26.5, gave, during rotation with the head inclined forward through 160 degrees, a circular nystagmus with the following values: 24.7, 35.6, 25.0. When the head was placed sidewise the results were 26.9, 51.7, 28.1. At the end of the practice series under 'normal' conditions, trials were again taken with the head bent forward and to the side with the following results:

TABLE XIV

	Head Forward				Head to Right Side		
	Time	No.	App. Mvt.		Time	No.	App. Mvt.
Aug. 8	12.2	14.4	14.0	Aug. 12	11.5	27.7	13.2
Aug. 9	12.2	12.5	14.0	Aug. 13	11.2	23.5	12.5
Aug. 10	8.1	10.7	9.7	Aug. 14	9.2	18.0	9.7
Aug. 11	7.4	11.2	8.2				

At the time of the initial trials, the nausea and dizziness were severe, but at the end of the 'normal' series, the subject

experienced no uncomfortable effects of the unusual position, showing that the effect of practice was more deep-seated than the ocular effects alone. Moreover, this subject and also other subjects have enjoyed an increasing ability to change the position of the head both during and after rotation without experiencing the tremendous increase in the intensity of the organic effects which usually accompany such a change in position.

V. Conclusions

We have discovered, then, the following facts about the organic effects of rotation, especially about the local effects called nystagmus. We have found that, as turning is repeated from day to day, the duration of the after-nystagmus, the number of ocular movements made, and the duration of the apparent movement rapidly decrease. The major part of this decrease occurs within the first few days. The decrease takes place not only from day to day but also within a period of ten trials on any single day. The amplitude of the ocular movements and the number of movements made per second also decrease as repetitions increase. Furthermore, certain other organic effects, especially those known as past-pointing, decrease in the same manner. We have also found that the time of nystagmus changes with the speed of rotation and with the number of revolutions and that it is increased when the chair is abruptly halted. As to other conditions under which nystagmus varies in degree and amount, we have found that we must consider: (*a*) the time of day during which rotation is carried on; (*b*) the amount and number of rest-intervals between turnings and between series; and (*c*) the general organic state of the subject. Finally, nystagmus may be modified indirectly by 'transfer.' In general, we have found the organic effects of rotation to be highly variable in their appearance and, moreover, so amenable to practice that they may entirely disappear within a relatively short time, provided rotation is repeated from day to day.

A SERIES OF FORM BOARDS

BY GEORGE OSCAR FERGUSON, JR.[1]

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I. *The Boards.*—In an attempt to devise a series of form boards of graduated difficulty of performance, eight boards of uniform size and of six holes each were designed. The boards were thirteen inches long, ten inches wide and one inch thick, and were stained light brown. The blocks were five eighths of an inch thick and were stained dark brown on both sides and on the edges. There were no pegs or handles on the blocks. The holes were one half inch deep, so that the blocks, when in position, projected one eighth of an inch above the surface of the boards and were easily removable from the holes.

All of the holes were of geometrical shapes and no two were alike. The principles employed to vary the difficulty of the boards were as follows:

1. Differences in the simplicity or complexity of the outline of the holes.

2. Some of the boards had only one block for each hole while others had two blocks for each hole.

3. In some of the boards the blocks could be fitted into the holes with either side uppermost; in other boards only one side of the blocks could be uppermost.

4. A given edge of a block could be either toward or away from the subject or to his right or left, when fitted into some of the boards; in other boards a given edge of a block had to face a definite direction with regard to the subject.

[1] The writer is indebted to Professor R. S. Woodworth for suggesting that a series of form boards be made and for access to the Columbia psychological workshop in making them; to Mr. A. S. Otis for constructive suggestions as to the scoring of the boards; to Messrs. O. R. Johnson, L. E. Becraft and W. B. Gibson for testing many of the subjects from whom results were obtained; and to his wife for assistance in making the computations.

5. Where there were two blocks for a hole, in some boards their positions were interchangeable within the hole while in others they were not.

6. The adjacent edges of two blocks in a hole were beveled in some boards while in others they were plain. The beveling was done by cutting, at an angle of 45 degrees to its surface, the original single block which fitted a hole, thus making two blocks with complementary beveled edges.

7. In some boards the two blocks in a hole were fitted to each other by means of a tongue and groove. The tongues and grooves were made by cutting, equally from each side, at an angle of 45 degrees, the original single block which fitted a hole. The two blocks resulting from the cutting thus had a tongue five sixteenths of an inch long fitting into a complementary groove.

8. In some of the boards where there were two blocks to a hole, certain of the blocks in different holes had the same surface shape, but differed in that one was tongued while the other was grooved or in that their edges were beveled in opposite directions.

These principles of varying the difficulty of the boards were combined in various ways in designing the series. Preliminary trials with a number of subjects showed that while all of the boards differed in ease of performance, four of them were comparatively easy while four were comparatively hard, the gap between the two sets of four being as great as the variation within either set. Four additional boards were then constructed, based upon the principles used in making the original eight, and it was found that these four boards were of such difficulty as to fill the gap existing between the two halves of the original set.

From the series of twelve boards six were finally selected to constitute the series described herein. These six were selected because of the regularity of their gradation from easy to difficult and because of their reliability as indicated by the distribution of their results. Fig. 1 exhibits their design.

II. *Procedure.*—All twelve boards were given to 170 subjects in the elementary grades and in college. The six

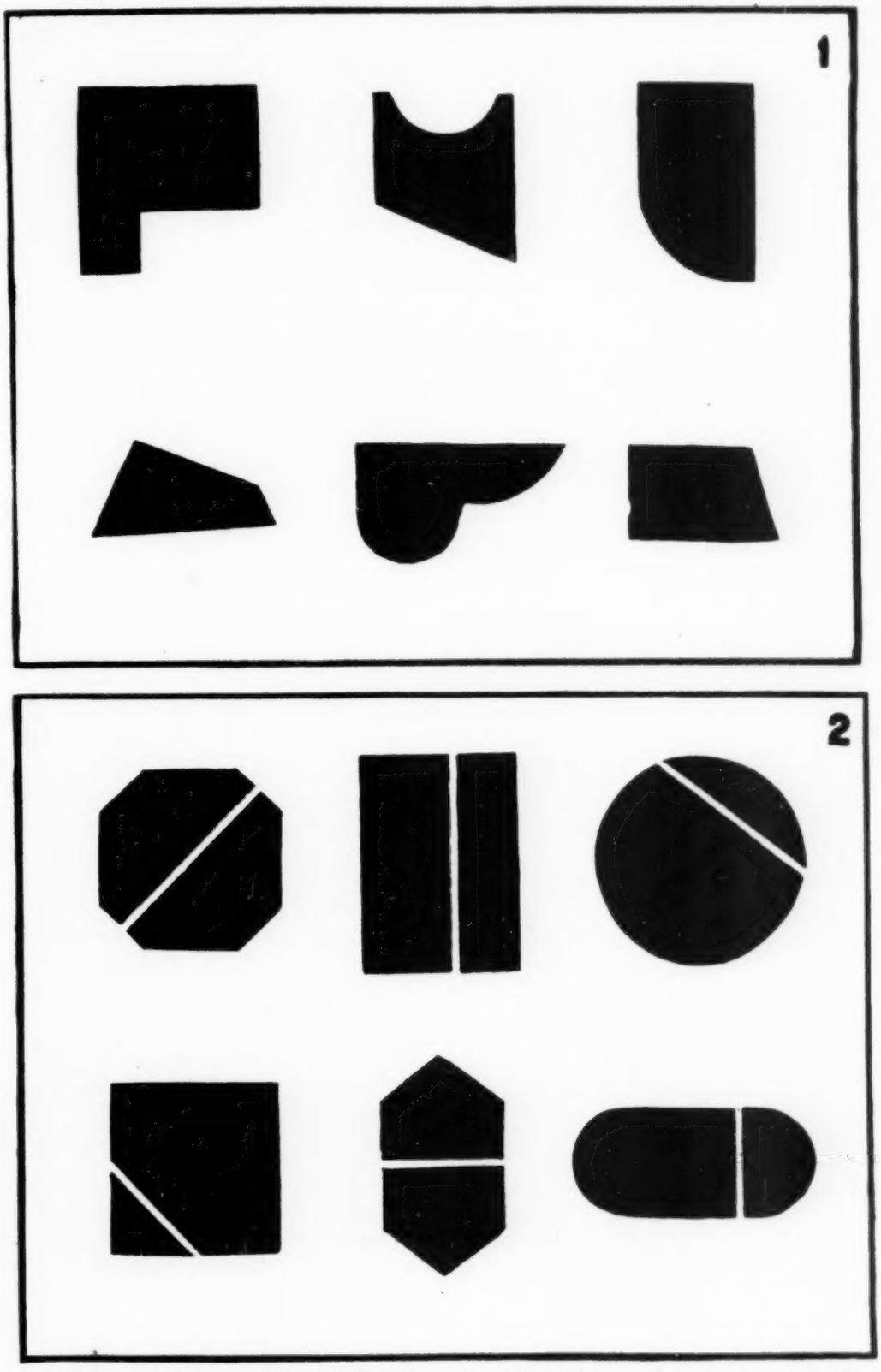

FIG. 1. Diagram of the Six Boards. *A* = block with overhanging beveled edge; *B* = block with beveled edge fitting under the overhang; *C* = block with projecting tongue; *D* = block with groove to fit tongue.

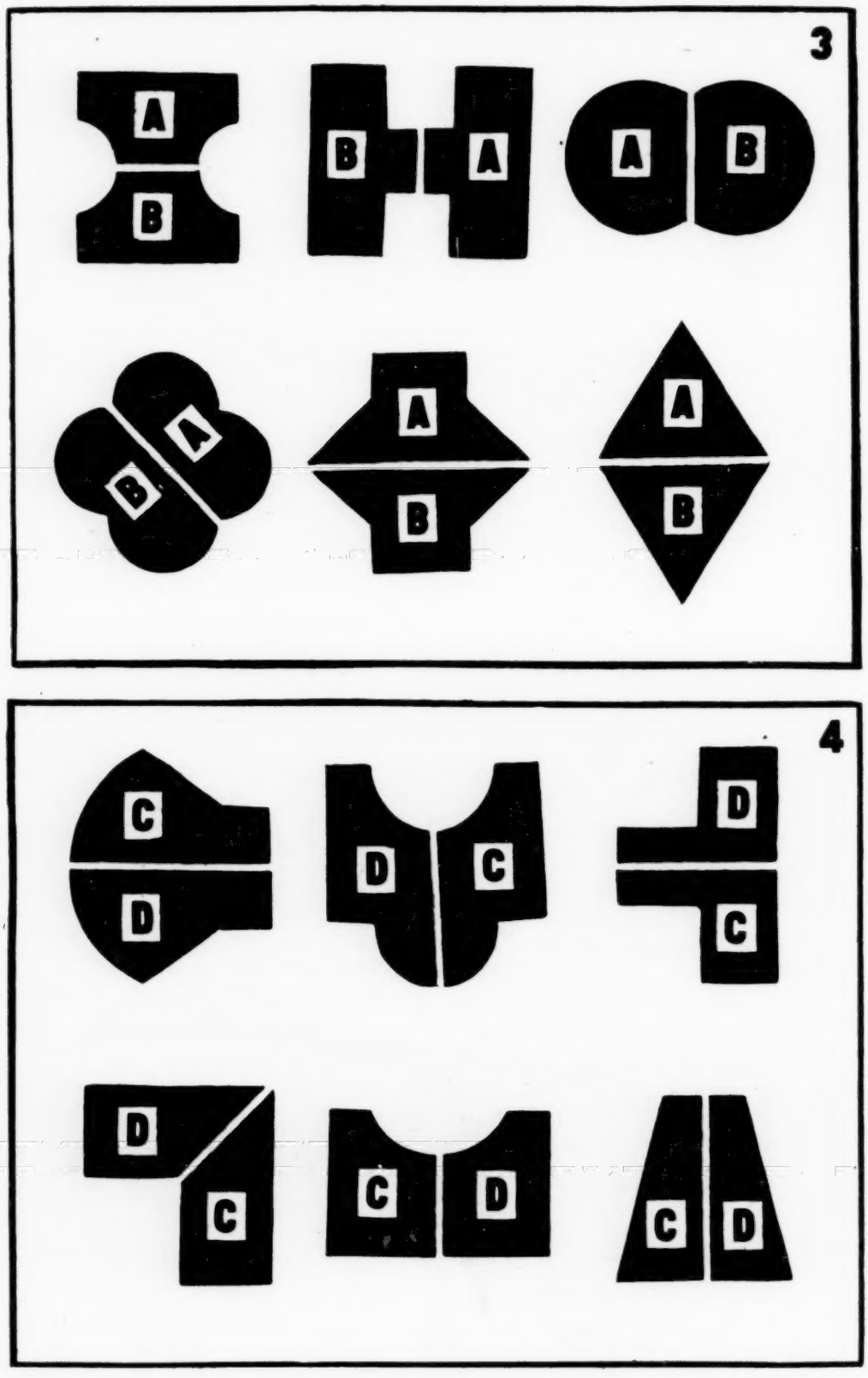

FIG. 1.—Continued.

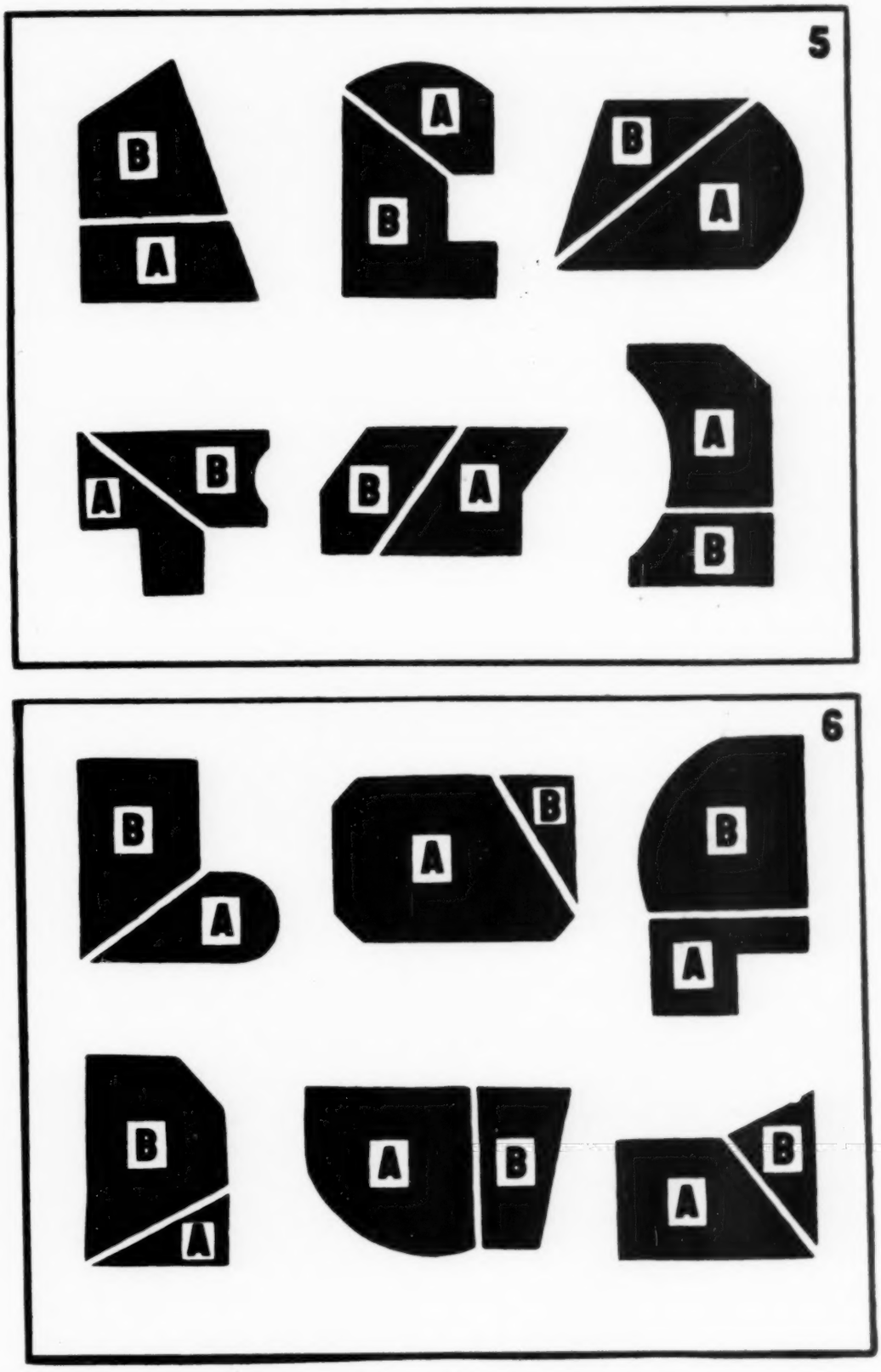

FIG. 1.—Continued.

boards finally selected were then given to 194 additional subjects in the elementary grades, high school and college. In all, 364 individuals were tested with the six selected boards. The number in each grade appears in Table II. Some of the elementary school children tested were in the public school of Williamsburg, Va.; others, with the high school pupils, were in the public school of Hamilton, N. Y.; the college students were in Colgate University. There was no discernible difference between the records of the Williamsburg and the Hamilton children.

The boards were always presented in a definite order, the easier first and the harder last. But it was found that the results for the six selected boards were the same when they were given in the series of twelve as when they were given as a series of six. Each board was presented separately, with the subject facing the lower side as the boards appear in Fig. 1. The blocks were arranged in chance order at the side of the board opposite the subject, who was allowed to choose his own position and to use one or both hands. In giving verbal directions the aim was to make sure that each child understood perfectly what was to be done, rather than to give stereotyped instructions. The directions were approximately as follows:

"I have here a board which has six holes cut in it. There is a wooden block (or there are two wooden blocks) in each hole. The blocks fit the holes exactly. I am going to empty all of the blocks out of the holes and place the board in front of you. When I lay the board down I want you to put all of the blocks in their proper places as quickly as you can. Do you understand? (Repeat directions.) Then begin."

III. *Scoring.*—The scoring was based entirely on the time required to place all of the blocks in the holes, no account being taken of errors or of partial completions. When the last block was properly placed the time was recorded. A maximum period of five minutes was allowed for each board. When five minutes elapsed without success, the correct solution was shown by the experimenter and another board was presented.

TABLE I

DISTRIBUTION OF THE SCORES FOR EACH BOARD IN EACH SCHOOL GRADE
Actual figures reduced to percentages.

Score	Grade															
	1	2	3	4	5	6	7	8	I	II	III	IV	F	S	J	S
								Board 1								
5	9	18	25	33	38	74	76	80	95	87	94	93	90	100	100	100
4	41	36	50	50	47	23	24	20	5	13	6	7	10	—	—	—
3	22	25	17	11	9	3	—	—	—	—	—	—	—	—	—	—
2	12	11	4	6	3	—	—	—	—	—	—	—	—	—	—	—
1	12	7	4	—	3	—	—	—	—	—	—	—	—	—	—	—
0	3	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—
								Board 2								
5	—	—	4	6	3	3	14	20	5	20	12	20	40	33	37	32
4	22	25	29	67	59	67	57	60	74	60	81	67	50	58	56	60
3	25	28	25	11	19	23	29	20	21	20	6	13	10	8	6	4
2	22	21	29	11	12	8	—	—	—	—	—	—	—	—	—	4
1	19	14	12	11	6	—	—	—	—	—	—	—	—	—	—	—
0	12	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—
								Board 3								
5	—	—	—	—	—	—	—	—	—	—	—	—	—	8	6	4
4	—	3	4	11	9	18	29	40	37	47	37	47	60	83	81	64
3	6	7	29	28	28	46	38	30	47	40	31	27	30	8	12	28
2	9	18	8	17	28	23	24	10	—	7	25	20	10	—	—	4
1	22	25	38	28	25	13	—	10	16	7	6	7	—	—	—	—
0	62	46	21	17	9	—	10	10	—	—	—	—	—	—	—	—
								Board 4								
5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
4	—	—	—	6	—	—	14	5	5	13	12	13	30	33	44	32
3	—	7	—	11	6	23	24	40	32	47	50	47	50	42	37	36
2	13	7	8	11	16	21	29	25	32	13	25	27	10	8	6	20
1	37	29	58	45	50	51	24	20	26	13	—	7	—	17	12	12
0	50	57	33	28	28	5	10	10	5	13	12	7	10	—	—	—
								Board 5								
5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
4	—	—	—	—	—	—	—	—	—	—	—	—	10	—	12	12
3	—	—	—	—	—	5	10	30	16	7	31	13	30	67	50	40
2	—	—	4	6	6	13	24	15	26	20	37	47	30	25	25	24
1	16	11	8	33	31	51	38	40	32	60	12	27	20	8	12	24
0	84	89	88	61	62	31	29	15	26	13	19	13	10	—	—	—
								Board 6								
5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4
3	—	—	—	—	—	3	—	—	—	7	—	—	10	17	12	12
2	—	—	—	6	6	8	10	10	11	7	6	20	10	25	50	40
1	—	—	4	11	12	26	33	40	21	33	37	40	50	33	31	16
0	100	100	96	84	81	66	57	50	68	53	56	40	30	25	6	28

A number of plans of scoring were tried. Attempts to express scores in terms of the actual time required to solve boards were unsatisfactory because of the practical necessity of setting a time limit. A record of the number of boards which were not solved within the time limit was unsatisfactory

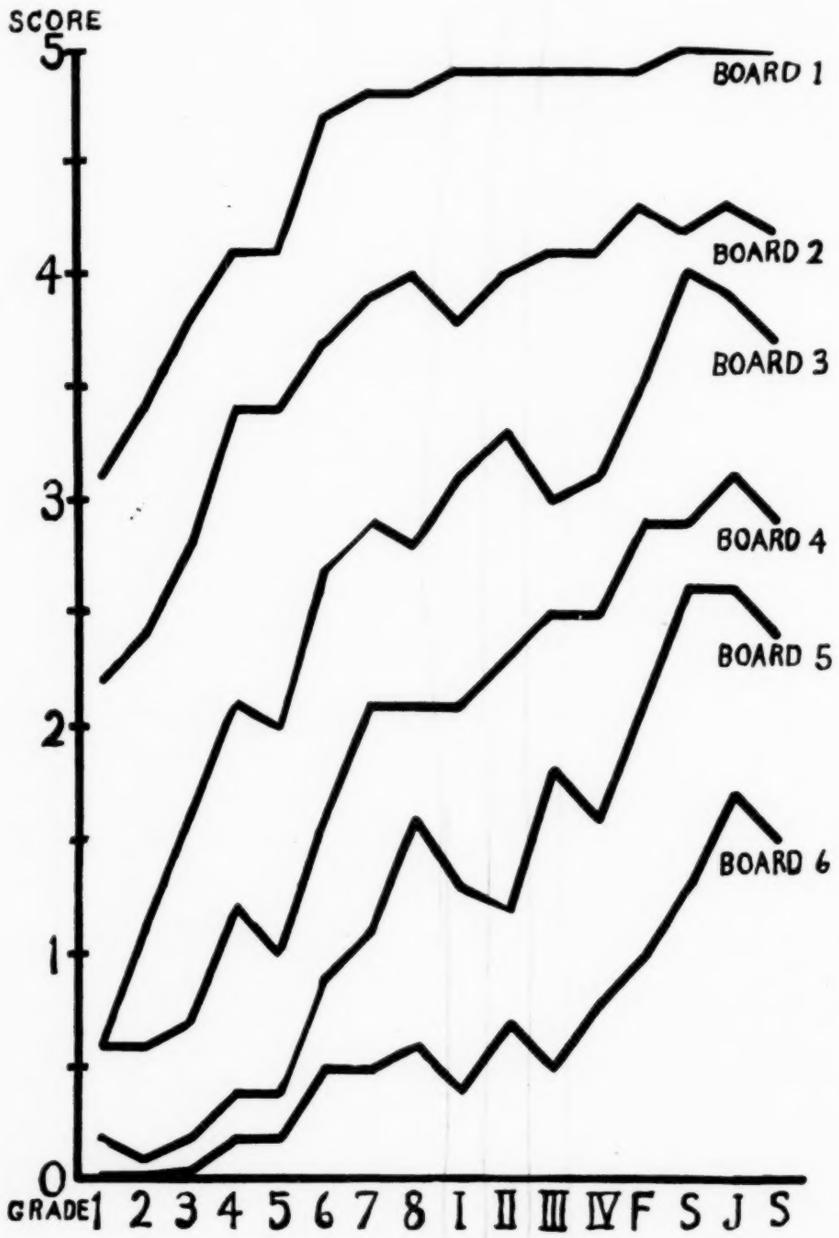

FIG. 2. Average Score for Each Board in Each School Grade.

as a score in that it did not distinguish between any except the most pronounced differences of ability. The plan finally

adopted was chosen because it gave the most nearly normal distribution of results and because of its simplicity. Each board was scored in the same way and the sum of the scores on the six boards was taken as the final score. A correct solution of a board in less than 30 seconds was scored 5;

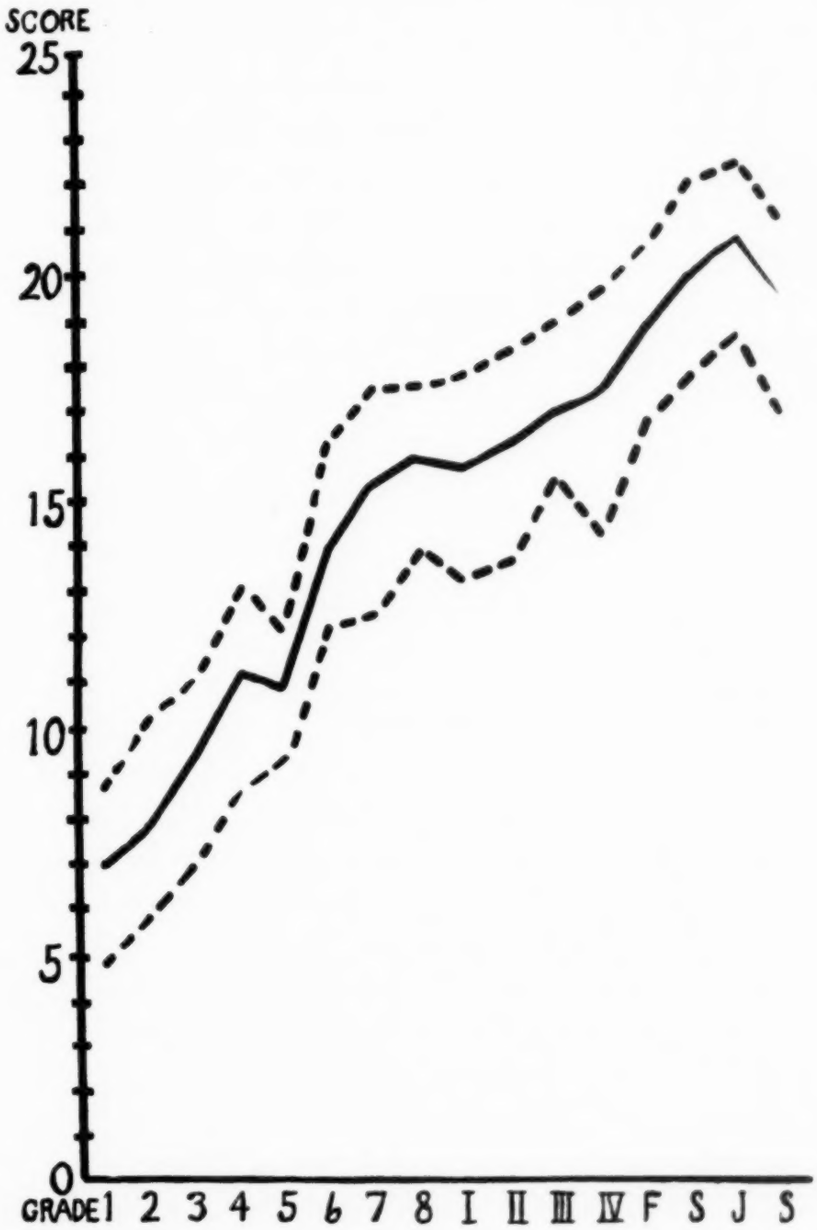

FIG. 3. Solid line = median score in each school grade for the series of six boards. Broken lines = quartiles.

a solution in less than 60 but more than 29 seconds was scored 4; a solution in less than 100 but more than 59 seconds was

scored 3; a solution in less than 150 but more than 99 seconds was scored 2; a solution in less than 301 but more than 149 seconds was scored 1; if a solution was not obtained in 300 seconds the score was 0. These values are shown in the following table:

Time	0	30	60	100	150	301
Score	5	4	3	2	1	0

The records obtained in the elementary grades, in addition to being scored by using the time intervals indicated, were also scored by using other intervals for the sake of comparison. In the fifth and sixth grades, containing 71 pupils, use of the intervals 0, 15, 30, 60, 120, 301 gave a coefficient of correlation of .98 with results from the intervals adopted. In these grades, intervals of 0, 60, 120, 180, 240, 301 gave scores which correlated .95 with scores obtained when the adopted intervals were employed.

IV. *Results.*—Fig. 2 exhibits the score obtained on each board by each grade. Table I. shows the distribution of the scores of each grade on each board. It is evident that the boards increase in difficulty from No. 1 to No. 6 and that the higher scores on each board are obtained by the higher grades.

Fig. 3 exhibits the median score and the upper and the lower quartiles for each grade on the series of six boards. The average distance of the quartiles of each grade from the grade median is 2.1 points of the scale; a range of 4.2 points includes 50 per cent. of the individuals in a grade. Table II. shows the detailed distribution of the scores. The relationship between score and grade expressed in Table II. gives a coefficient of correlation of .81. This is as high as the coefficient obtained between Alpha, Beta or Stanford-Binet scores and reported school grade when these tests were given to unselected drafted men at Camp Lee. It may be noted that the median form board score for each school grade is roughly the same as the median age of the grade.

Thirty-six of the pupils of the sixth grade in the Hamilton school, who had been tested with the form board series, were given the army Alpha test and were rated by their teacher,

who had taught them for six months, for intelligence and for class standing. The rating was in percentages, since that was the method in regular use in the grade. Coefficients of correlation of .51, .50 and .56 were obtained between the ratings in Alpha, estimated intelligence and class standing, respectively, and score in the form board series. Alpha gave coefficients of correlation of .73 and .68 with estimated intelligence and class standing, respectively, and estimated intelligence and class standing correlated .90 with each other.

TABLE II

DISTRIBUTION OF THE SCORES IN EACH SCHOOL GRADE FOR THE SERIES OF SIX BOARDS

Score	Grade															
	1	2	3	4	5	6	7	8	I	II	III	IV	F	S	J	S
25																1
24														1	2	
23													1	3	1	2
22									1			1	2	2	2	1
21							1			2	1	1	1	3	2	4
20								1	1		2	1	3	3	3	2
19						1	1	1	1		1	2	2	4	1	5
18						2	1	2	1	2	3	2	3	1	3	2
17					1	2	4	3	4	1	1	1	2	4		2
16				1		5	2	3	1	3	3		3	2	1	4
15				1	1	6	2	3	3	1	2	2	1	1	1	1
14			1	1	2	3	1	1	1	2	1	2	1			
13		2		1	1	6	3	3	2	3	1	1	1			
12	1	2	2	2	4	5	2	1	2							1
11	1	2	3	4	7	5	1		1	1	1	1				
10	2	1	2	2	6	2	1		1			1				
9	3	4	5	2	3	1	1	1								
8	5	2	4		3	1		1								
7	4	3	1	1	1		1									
6	3	4	3	2	1											
5	4	3	1	1	1											
4	3	1	1		1											
3	2	1	1													
2	3	2														
1	1	1														
0																
No.	32	28	24	18	32	39	21	20	19	15	16	15	20	24	16	25

Sixty negro children, fifteen selected by chance from each of the first four grades of the Williamsburg colored school, were tested with the form boards with a view to comparing their scores with those of sixty white children similarly selected from the first four grades of the white school of the same city. The comparison showed that 33 per cent. of the negro children

equaled or exceeded the median score of the white. In such tests as the ordinary completion, analogies or other tests of controlled association involving verbal symbols, only about 20 per cent. of negro elementary school children equal or exceed the median score of whites in the same grades.

When the performance of the girls in the form board series was compared with that of the boys, grade by grade throughout the elementary school, it was found that 48 per cent. of the girls equaled or exceeded the median of the boys.

V. *Summary.*—The six boards of the series increase in difficulty from No. 1 to No. 6 by fairly constant increments. There is also a fairly constant rate of increase in the scores obtained by successive school grades on each of the boards. The series as a whole appears to be adapted to the testing of individuals throughout the school system, from the primary grades to college. The median score obtained by a given grade is roughly the same as the median age of the grade; within a given grade the median deviation is about two points of the scale.

The basis of scoring is the time required to fit the blocks into the boards. The boards are presented one at a time, beginning with the easiest, and the subjects are instructed to fit the blocks into the holes as quickly as they can. Solutions of a board within time limits of 30, 60, 100, 150 or 300 seconds give scores of 5, 4, 3, 2 or 1, respectively. If a board is not solved within five minutes the score is zero. The sum of the scores on the separate boards constitutes the total score.

The results obtained indicate that boys do as well as girls on the boards, or perhaps slightly better; that about 33 per cent. of negro children score as high as the median of white children; that within a given school grade performance on the boards correlates approximately .50 with class standing, estimated intelligence or army Alpha score; that performance on the boards correlates about .80 with school grade.

AN APPARATUS FOR DETERMINING ACUITY AT LOW ILLUMINATIONS, FOR TESTING THE LIGHT AND COLOR SENSE AND FOR DETECTING SMALL ERRORS IN REFRACTION AND IN THEIR CORRECTION

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This apparatus was devised in response to a request by the Eye Division of the U. S. Naval Hospital for a means of making a quick and accurate test of acuity at low illumination. Experience has shown, roughly speaking, that only 25–30 per cent. of the men on the battleships have a sufficiently keen acuity at low illuminations to qualify for all branches of the lookout and signal service work. The apparatus provides for a wide range of illumination in just noticeably different steps (beginning at 0.07 meter-candle or lower), with no change in the color value of the light and with a specification at each step of the intensity of light falling on the test-object.

Among the requirements for an apparatus for determining acuity at low illuminations or the effect of change of illumination, the following points may be mentioned. (1) A means of changing the illumination by small amounts over a wide range, beginning at or below the threshold for the test-object employed, without changing the color value of the light. If in making this change the color value of the light is altered it is obvious that another factor affecting the results is introduced. (2) A means of keeping constant for an indefinite length of time any desired intensity of illumination and of reproducing this intensity at will. Also the test-object must be uniformly illuminated. (3) A means of specifying accurately at any point in the scale the intensity of light falling on the test-object. And (4) it is desirable that the apparatus employed for controlling the illumination can be used with the test-objects already accepted in clinic practice.

The most difficult problem one has to face in constructing an apparatus for determining the minimum amount of light that permits of the discrimination of a given test-object, more particularly if that object consists of a line of test letters, is to secure a uniform illumination of the line. This problem is relatively unimportant at the illuminations ordinarily used in acuity testing, because at these illuminations acuity changes so slowly with chan e of intensity of light that the differences which may occur throughout the line of test letters do not ordinarily produce a detectable effect on the results of the test. However, if no more care is exercised at the threshold to secure uniformity of illumination than is ordinarily used at the higher illuminations, no single intensity at the source will serve for the discrimination of all of the letters of the line. We were able satisfactorily to meet this difficulty in only one way, namely, by selecting an aperture sufficiently small to permit of its uniform illumination and projecting a magnified image of this aperture on the test card. That is, an aperture was selected of such a size and shape that when magnified fivefold it gave a band of light which just blocked off one line of the test letters. It is obvious that this aperture could be made of different sizes and shapes, depending upon what is wanted in the projected image. For example, two or three lines of test letters could be blocked off if desired, or the whole card or any part of it could be illuminated, etc. There is no reason, moreover, why the aperture could not be made adjustable in size to suit the needs and preferences of the individual operator. In one model of the apparatus these apertures were cut in a series of slides which could be inserted in the projection tube just outside the lamp house in grooves in a light-tight boxing. A convenient means was thus provided for changing the aperture, if desired, during a series of tests without having to open the lamp house. The source of light is a well-seasoned Mazda C lamp of the round bulb or stereopticon type of 100, 250 or 500 watts, depending upon the range of illumination that is desired. This lamp is installed vertically in the roof of the lamp house at such a height that its filament is well above the aperture which is

to be illuminated. In order to secure a uniform and diffuse illumination of the aperture the lamp house is lined with opal glass ground on one side. The aperture, 6 x 1 cm., is cut at the center of the cap covering the inner end of the projection tube. Further to aid in the even illumination of the aperture, it is covered with a slide of ground glass. To prevent the overheating of the lamp house, a rather elaborate ventilating system is provided consisting of a light-tight ventilating hood at the top and a series of holes on two sides at the bottom of the housing, furnished with light-tight shields. The changes in the intensity of light are produced by means of an iris diaphragm. When such a diaphragm is placed either at the front or back surface of the focussing lens, changes in the flux of light can be produced without any alteration in the size or the shape of the image produced by the lens, just as happens, for example, in the action of the iris of the eye. At a suitable point in the circumference of the diaphragm is fastened a pointer which, as the diaphragm is opened and closed, moves over a translucent millimeter scale. This scale is mounted over a slot in the projection tube and receives its illumination from the light inside of the tube. The inside of the tube is painted a mat black. At the further end of the projection tube, 18.1 cm. from the illuminated aperture, in a brass ring and collar is mounted the focussing lens. This lens is 7.5 cm. in diameter and has a focal length of 14.8 cm. A different strength of lens could have been used and different relative distances of aperture and test card from the lens. With appropriate variations in these factors the distance of the lantern from the test card and the amount of magnification of the projected image may be varied. Any increase of magnification results of course in a decrease in the brightness of the image, hence an increase in the scale of brightness of image with no change in its size could have been obtained by increasing the size of the aperture and decreasing correspondingly the amount of magnification. In the construction of the present apparatus a 14.8 cm. focal length lens was used because it could be obtained the most readily on the market of the diameter needed. On the plat-

form supporting the lamp house are mounted a small Weston ammeter and a small rheostat to guard against fluctuations in the current and consequent fluctuations in light intensities. In order that any line of the chart may be illuminated at will, the lamp house is mounted on the end of a rod which is raised and lowered by means of a rack and pinion. The test card is mounted at a distance of 81 cm. from the focussing lens. A photograph of the apparatus is shown in Fig. 1.

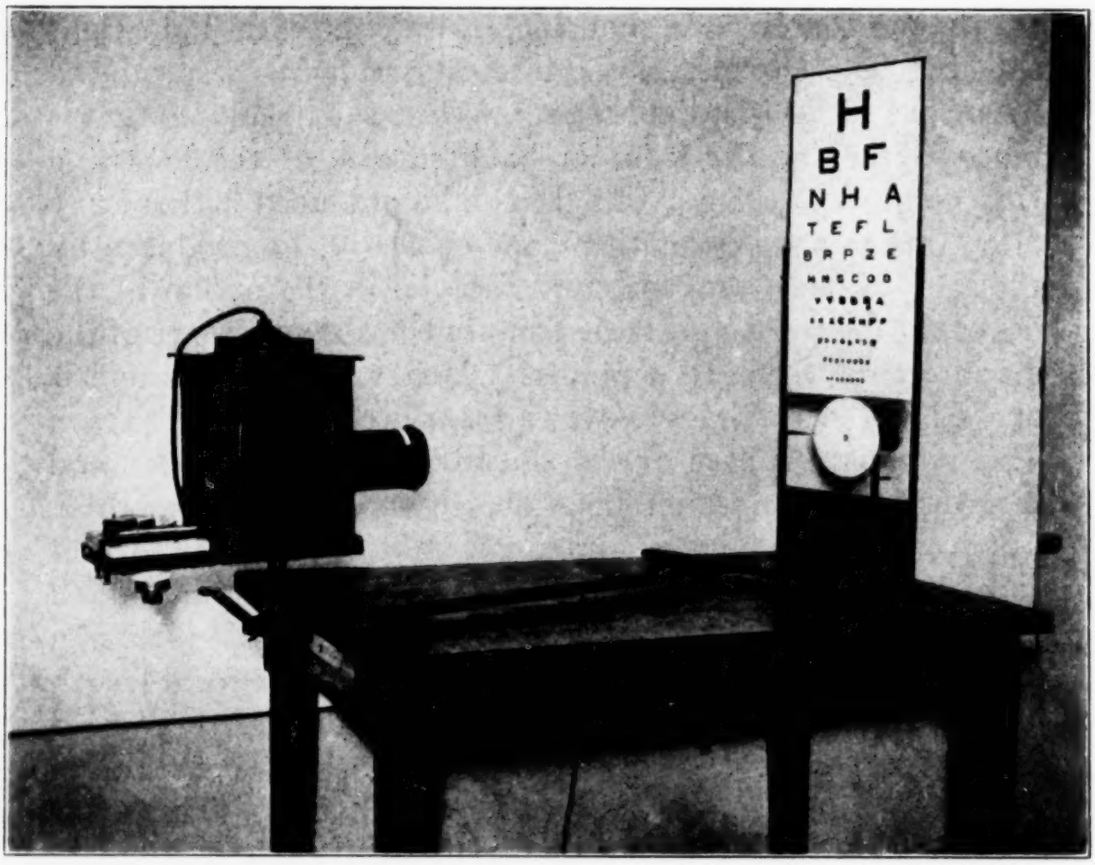

FIG. 1.

In order that the intensity of light used at any time may be known, a calibration chart is provided in which are given the readings on the millimeter scale and the equivalent meter-candle values at the test card. This calibration was accomplished as follows: The lamp house was removed and mounted on a photometer bar at a distance from the photometer head equal to its original distance from the test card. The scale

was then gone over point by point and the meter-candle value of the light at the photometer head was measured. The calibration chart is shown in Fig. 2 *a*. In Fig. 2 *b* is shown the calibration curve in which the divisions of the millimeter scale are plotted against meter-candles at the test card. These values are corrected to conform at the center of the card to the cosine law.

Diaphragm Setting	Meter-candles	Diaphragm Setting	Meter-candles
82.5	9.19	46.0	2.46
82.0	9.04	44.0	2.23
80.0	8.54	42.0	2.02
78.0	8.06	40.0	1.81
76.0	7.58	38.0	1.61
74.0	7.12	36.0	1.43
72.0	6.67	34.0	1.27
70.0	6.23	32.0	1.12
68.0	5.81	30.0	0.97
66.0	5.43	28.0	0.82
64.0	5.06	26.0	0.69
62.0	4.71	24.0	0.58
60.0	4.36	22.0	0.48
58.0	4.02	20.0	0.38
56.0	3.72	18.0	0.28
54.0	3.45	16.0	0.21
52.0	3.18	14.0	0.16
50.0	2.93	12.0	0.11
48.0	2.69	10.0	0.07

FIG. 2*a*. Calibration Table.

For our own use in the laboratory we have preferred to substitute for the Snellen chart a single test character, the broken circle (the international test object), which can be turned in different directions and the judgment of its direction rather than the recognition of the character be required of the observer as a test of discrimination. Our reasons for this preference are as follows: (1) A test letter may be recognized when it is not seen at all clearly. Recognition is too dependent on extraocular functions to be used with precision as a measure of ocular capacity. (2) The different letters of the Snellen chart set an unequal task for the resolving power of the eye. (3) An objective check is had on the judgment. This is especially helpful in case of children, and the unintelligent, untrained and subjective type of adult. (4) By the use of the same test character, turned in different direc-

tions at will, all possibility of learning the test series is eliminated. Also the test-object becomes much more valuable for the detection of astigmatisms. And (5) at low

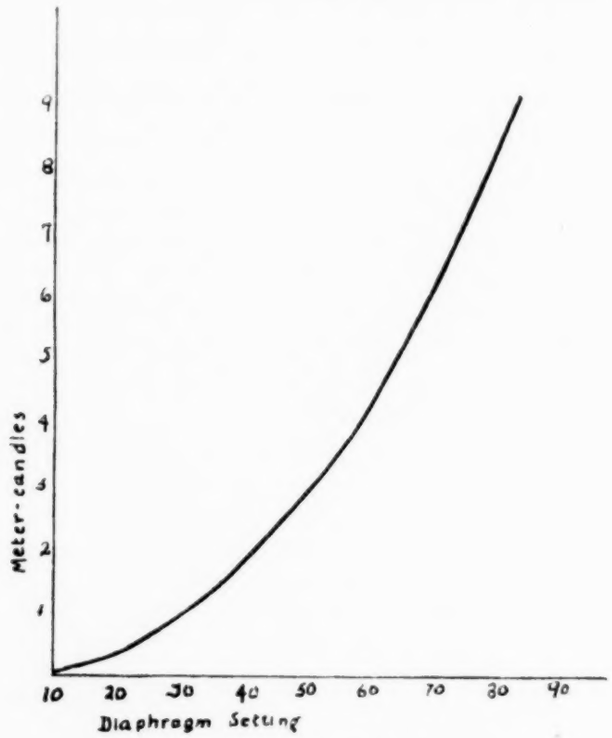

FIG. 2*b*. Calibration Chart.

illuminations the eye fatigues very rapidly. Thus if the task is the reading of the whole line of letters the results obtained measure not only acuity, but the power to sustain acuity which may or may not be compatible with the purpose of the test.

As stated in the introduction, the apparatus was designed to meet a specific testing need of the Navy. However, it has in addition the following laboratory and clinic uses. (1) Photopic acuity may be tested under the conditions of a constant and uniform illumination of known intensity. In case the test-object is a line or chart of letters, provision is made that each letter receives, within sensible limits, equal amounts of light. (2) Scotopic or twilight vision may be tested—also the amount and rate of scotopic adaptation. A precise and feasible means is thus afforded for testing the light sense insofar as it affects the power to see clearly. (3)

If the image of the aperture is projected on a blank white surface of good reflecting power, more particularly if its size and shape are changed to that of a square or circle of dimensions favorable for making a sensitive judgment, the apparatus can be used with an equal degree of precision and convenience for testing the light sense directly in terms of the amount of light required to arouse just noticeable sensation. That is, the threshold of sensation can be determined in terms of meter-candles of light falling on the test surface; or, knowing the coefficient of reflection of this surface and the breadth of pupil, in terms of the amount of light entering the eye. The testing of the light sense will probably always remain foundation work in the clinic routine. The testing of scotopic acuity, for example, is not sufficiently differential in all cases between refraction defects and the hemeralopias and other retinal deficiencies to serve as a satisfactory substitute. There is, at present, we are told on very good authority, no satisfactory instrument for testing the light sense available to the ophthalmologist. And (4) by making it possible to determine with great exactness the minimum of illumination at which the test-object can just be discriminated, the apparatus provides a very sensitive means for detecting small errors in refraction and in their correction, as will be demonstrated in a later paper.

Sensitivity for detecting small errors in refraction and in their correction could also be added to the acuity test by using a test-object, small changes in the size of which could be made. However, no means has as yet been provided for making small changes in the size of any acuity object sufficiently complicated in form to test simultaneously the resolving power of the eye in any great number of meridians which is, we believe, a very important feature in determining the exact location of an astigmatism or the exact amount and placement of its correction, more particularly when a cycloplegic is not used.

We consider this an important feature in testing for an astigmatism because of our belief that the astigmatic eye in the attempt to compensate for its defect has in many cases

at least acquired unusual powers and habits of accommodation. Our belief in this is based on three sets of observations. (1) In the use of the astigmatic charts without a cycloplegic in cases of low astigmatism, one is frequently annoyed by the astigmatic indication shifting from one meridian to another. (2) We have found observers who could voluntarily, in some cases requiring considerable practice, shift the meridian showing the astigmatic indication. And (3) in our test of astigmatism based on the relative speed of discrimination in the different meridians, *American Journal of Ophthalmology*, 1918, I., pp. 3–16, the speed of discrimination for the least favorable meridian can be increased by practice almost to equal that in the most favorable meridian with an equal amount of practice. This result is so noticeable that in order to make the test sensitive we were compelled to eliminate as much as possible the opportunity for the practice effect. This was done in two ways. (1) The series was always begun below rather than above the minimum time of exposure required just to detect the direction in which the test-object was turned. And (2) meridians were inserted in the series clearly outside of the region of maximum astigmatic effect in order to break up any progressive tendency to accommodate especially for the meridian showing the poorer resolving power. The fact that the eye can with long exposures discriminate a fineness of detail in its unfavorable meridian which it is utterly unable to master with short exposures and that this excess lag can be overcome in a considerable measure by practice seems to indicate that it has the power through its active accommodation to overcome in part the effect of meridianal inequalities in resolving power, at least when a test-object taxing the resolving power in only one meridian is turned successively into different meridians. In any event it seems only the part of sound procedure in testing without a cycloplegic to guard against the possibility of selective meridianal accommodation by the use of a test-object which taxes the resolving power of the eye in as many meridians as possible.

But even if a test-object complicated in form and minutely

adjustable in size were available, a device for determining the minimum illumination at which the test-object subtending the standard visual angle can just be discriminated would afford a still more sensitive means for the detection of low astigmatisms and small errors in the amount and placement of their correction. This follows rather obviously from the fact that for all but very low intensities acuity changes slowly with change of illumination. That is, for all but very low intensities small differences in acuity correspond, comparatively speaking, to large changes in illumination. Used as we have recommended, the illumination scale becomes in effect an amplified indicating scale by means of which the relatively slight differences in acuity, represented by the proper correction of an error in refraction and small deviations therefrom, may be detected with comparative ease and certainty. It is not infrequent perhaps to find that in cases of low astigmatism, with the full illumination of a test-object presenting no smaller gradations in visual angle than are found in the Snellen chart, the observer is able to detect no difference in the ease or clearness of discrimination of the test character through a range of from 20–40 degrees in the placement of the correction. This difficulty is especially annoying in the case of children and the unintelligent, untrained and subjective type of adult. In such cases the apparatus shown here is especially helpful. With it a minimum is left to the comparative and observational powers of the subject. All that he is required to do is to indicate the direction in which the test-object points, the most favorable amount and placement of the correction being determined by the minimum amount of illumination at which he is able correctly to give this indication. The apparatus possesses ample sensitivity, as our results will show, for the detection of an error of 5 degrees and less in the placement of the correction of a low astigmatism or of 0.12 diopter and less in the amount of the correction.

In a table to be given in a later paper it will be shown, for example, that an error of 5 degrees in the placement of the correction of an astigmatism produced by a 0.25 diopter cylinder

required as an average for five eyes 66.5 per cent. more light for the discrimination of the test-object than the correct placement; in case of an astigmatism produced by a 0.75 diopter cylinder it required 107.2 per cent. more light than the correct placement. The large number of scale divisions between the settings of the light control for the correct and incorrect placements of the cylinder will be shown also in these results. In case of the 5-degree displacement of the correction of 0.25 diopter astigmatism this difference averaged 9.6 for the five eyes. Since the apparatus can readily be set to half divisions, 19 settings of the light control could have been made with precision between the values needed for the true correction and the 5-degree displacement. This shows that the sensitivity of the apparatus far exceeds the present possibilities of the precise manipulation of the correcting cylinders.

In case of an error of 0.12 diopter in the amount of the correction, 54.6 per cent. more light was required for the least favorable meridian; and in case of an error of 0.25 diopter, 108.9 per cent.

The relation of the illumination scale to the detection of small errors in refraction and in their correction may be stated briefly as follows: Insofar as the test-object is concerned, clearness of seeing depends upon the value of the visual angle subtended and the intensity of the illumination. It follows from this that either the illumination scale or the visual angle scale may be used for the detection of errors in refraction, *i.e.*, in the diagnostic procedure either the illumination may be held constant and the visual angle varied, or the converse. Since the visual angle scale sustains by convention a 1 : 1 relation to acuity while acuity changes slowly with change of illumination for all but very low intensities, the illumination scale possesses the greater sensitivity for the detection of small errors in refraction—also the greater ease and feasibility of contrivance and manipulation. Used in this way the illumination scale becomes in effect an amplifying scale—somewhat analogous to the use of the tangent scale in detecting small deflections in the magnet system of a

galvanometer—and has an advantage in sensitivity in proportion to the amplification. In clinic practice it has been shown to be of particular value in determining the exact amount and placement of the correction of astigmatisms. That is, if the eye has equal resolving power in all meridians, the amount of light required just to discriminate the test-object in all meridians will be the same; if the resolving power is not equal, the amount of light required will be different in the different meridians and different by an amount proportional to the amplification represented by the illumination scale. A more detailed discussion of a feasible and convenient means of using the illumination scale in office and clinic practice will be given later.

Attachment for Testing the Light and Color Sense

A consideration of the foundation principles of the acuity apparatus reveals at a glance that they lend themselves readily to light and color sense testing for clinic purposes. In order to convert the apparatus in the form described in this paper into a light sense tester three features are needed: (*a*) the choice of an aperture such that when magnified fivefold a stimulus is obtained of a size and shape suitable for a sensitive judgment of the threshold of sensation; (*b*) the provision of a suitable surface on which to project the magnified image of the aperture; and (*c*) a means of reducing the intensity of light from the acuity threshold to the light sense threshold, *i.e.*, from the amount needed just to discriminate the standard acuity object to the amount needed just to arouse the light sensation. The iris diaphragm used in the present form of apparatus, range of pupil 5–65 mm., does not provide for this range of intensity without changing the source of light. It is obvious that an attachment for the further reduction of the light which does not interfere in any way with the use of the apparatus for the acuity work, would afford a more convenient means of securing the lower intensities than the changing of the source of light. Provision has been made for this in two ways: (*a*) by neutral absorption screens or filters; and (*b*) by a Nicols prism (polarizer and

analyzer). The attachment is made so that it will hold either of these reducing agencies, leaving the operator an option as to which shall be used. The filter holder is made from three grooved metal strips, 8 cm. long and of appropriate width and thickness, built into a three sided rectangular figure open at the top. It is fastened to a narrow collar which slips over the end of the projection tube of the acuity apparatus and is held in place by a set screw. The holder is provided with three grooves into which one, two or three filters 8 x 8 cm. may be inserted as desired, or the metal plate which holds the Nicols prism. The Nicols prism is mounted in telescoping tubes in the customary manner for reducing light intensities, one tube containing the polarizer and the other the analyzer. At the end of the tube containing the analyzer is a large milled head by means of which very small angles of rotation may be made. The angle of rotation is read by means of a graduated dial, 6 inches in diameter, and an indicator with a Vernier scale, attached respectively to the tubes containing the polarizer and the analyzer on either side of their junction. The tube containing the polarizer is firmly mounted in a brass plate, 8 x 9 cm., with its axis coincident with the normal to the plate at its center. When the Nicols is to be used instead of the filters, this plate is inserted in one of the grooves of the filter holder. So inserted its axis is in the principal axis of the projection lens of the acuity lantern and the inner end of the polarizer is in contact with the outer surface of the projection lens. When the filters are employed to reduce the light they are inserted in the holder to give the large initial cut down and the further graded reduction is made by the iris diaphragm of the acuity lantern. When the Nicols is employed, the iris diaphragm is set at its minimum aperture, 5 mm., and the further reduction is made by the Nicols and read from its scale.

The testing of the color sense is provided for by inserting color filters in the beam of light. These filters may be inserted at the illuminated aperture; in the filter holder in front of the iris and lens; or, with a slightly different con-

struction of projection tube, back of the lens as near to the iris as possible. The simplest of these possibilities, from the standpoint of the construction and operation of the apparatus, is to insert the filter in the holder immediately in front of the lens and cut down the light intensity by means of the iris diaphragm. If it should be desired or considered technically more correct, however, to produce the changes in intensity after the light has been passed through the filter, this result can be accomplished either by inserting the filter at the illuminated aperture or anywhere in the projection tube back of the iris, or by placing it in the holder in front of the lens, with the iris held constant, and changing the intensity by means of the Nicols prism.

Color sense apparatus for clinic purposes seems at present, so far as the central field is concerned, to be limited to the testing of such gross deficiencies as are classed as color blindness. They are of little use for detecting the smaller changes which mark the advance and recession of many pathological conditions. The present apparatus is designed for detecting and measuring the degree of deficiency in terms of the amount of light of a given range of wave-lengths which is required just to arouse the color sensation.

REACTION-TIME SYMPTOMS OF DECEPTION

BY

WILLIAM M. MARSTON

I. Problem

The experiment hereinafter reported was performed in the Harvard Psychological Laboratory during the Academic Year 1913–1914. At that time the writer of the present article, at the suggestion and under the direction of Professor Hugo Münsterberg, began experiment upon what was then planned to be a series of psycho-physiological problems in the field of legal testimony. The first psycho-legal problem to be approached was the investigation of the psycho-physiological symptoms of the deceptive consciousness. The systolic blood pressure symptoms of lying were reported upon by the present writer in the Journal of Experimental Psychology for April, 1917. Since that time considerable work has been done by the present writer and others upon deception tests in connection with the various psychological tasks undertaken for the Federal Government during the recent war. The present writer, in returning to the further laboratory investigation of the deceptive consciousness, is led to believe in the light of above-mentioned Army deception test experiments, that the practical value of psychological studies in this field lies almost wholly in a complete and comprehensive scientific discovery and analysis of all the psychological symptoms of deception rather than in attempted use of one isolated set of these symptoms for detection of deception on the part of witnesses or criminals.

A glance at the legal situation becomes necessary in pointing the practical problem. Mr. Bielaski, active head of the Bureau of Criminal Investigation for the Department of Justice during the war, expressed the opinion that the future of all deception tests lies in the possibility of their introduc-

tion as a basis for expert testimony, just as the various neuro-psychiatric tests are now used as the basis for testimony by alienists in cases of alleged insanity. In order to qualify a psychologist as an expert upon deception a psychological "patent medicine" is not sufficient. The court must be convinced that a sufficiently sound and fundamentally scientific knowledge of all the psychological symptoms of deception are available to enable the alleged expert, not only to give his opinion to the jury, but also to answer hypothetical questions put to him under cross examination, showing upon what state of psycho-physiological fact said opinion has been based. A "patent medicine" is a secret formula known only to the owner of the copyright and to the United States Bureau of Patents. The maximum value of many such proprietary remedies lies in the suggestion exercised upon the mind of the patient by the mystery of the contents, with the resulting apparently miraculous effect of the remedy upon the disease. It is to avoid exactly this suggestion of the miraculous or mysterious upon the minds of juries that our present trial systems demand, not only expert testimony, but the qualification of the expert by proof of the existence of a commonly known and recognized body of scientific fact upon which the expert bases his opinion. Thus, it is extremely doubtful whether a single deception test, such as the association reaction time test, the Benussi breathing test, or the systolic blood pressure test, would be admitted as a basis for expert testimony in court without a broader foundation of psychological fact than is at present available. In short, the court will not be satisfied to be assured by psychological authorities that this or that deception test does detect deception; many medical authorities might similarly testify as to certain patent medicines. The courts, through long experience in the analysis of testimony, are not looking for patent medicines; they want a free and common knowledge of all the ingredients.

We must, therefore, seek objective, quantitative measurements of the psycho-physiological symptoms of the deceptive consciousness. If a psychologist has to go on the stand and

testify that, in his opinion, the alibi of a defendant indicted for first degree murder is a lie, it seems obvious that no qualitative analysis of the symptoms during testimony will be sufficient. The quantitative analysis may indeed show only relative difference or significance but, nevertheless, quantitative measurements are in themselves definite and susceptible of common knowledge. The immediate problem of the experiment here reported was to study the reaction times of the subject during deception. Analysis of reaction time association experiments and tests reported in the literature shows that judgments have been based jointly upon the qualitative and quantitative aspects of the results. The qualitative aspect of these tests lies in the association reaction words, and the quantitative in the reaction times recorded. Our problem in the present experiment was to eliminate the words and isolate, if possible, the quantitative measurements. In all previously reported tests no unanimity of qualitative results has been reported but, on the other hand, so great a unanimity of quantitative results has generally been found that psychologists seem to have accepted as established the thesis that deceptive emotions tend to increase both association reaction times and mean variations. To test this thesis is the particular problem of the present experiment.

II. Method

Ten different subjects were used in the principal experiment; six subjects were graduate students of psychology who might properly be designated as trained subjects, and four were undergraduates of competent general psychological training but with no specialized laboratory experience. The method adopted purposed the exclusion of all mental elements extraneous to the interaction of deceptive consciousness and reaction times.

Series A.—Pasteboard index cards were prepared, carrying upon one side two columns of words, ten words to the column, printed in parallel columns on one side of the card. The cards were placed blank side up before the subject who was instructed at a given signal to turn over the card and give

an association word with each word in one of the lists on the card as fast as he could. The two columns of words on a card were called L. and R. (left and right) and the two opposite directions in reading each list were called U. and D. (up and down). The cards were presented to the subject in groups of eight. Before giving the signal for the subject to turn over each card, the experimenter instructed the subject to choose either the right or the left column on that card and to proceed through that column either up or down. In any four cards out of each group of eight the subject was to obey the experimenter's instructions and in the other four out of that group the subject was to reverse instructions by taking the opposite list and proceeding in the opposite direction. The subject was to deceive the experimenter as to the identity of the cards where instructions were reversed by proceeding to make his associations with the list on these cards as rapidly as on the cards where he obeyed instructions. The experimenter, of course, did not know which of the lists were done truthfully according to direction, and which were done deceitfully against direction. The truthful cards were called C. (correct) and the deceitful cards O. (opposite). At the end of each group of eight cards the experimenter attempted to judge by means of the reaction times alone which four lists the subject had performed C. and which four O. The reaction times were taken with a stop watch operated by a magnet; the circuit being closed first by a Dunlap voice key, but later, when this proved unsatisfactory, by a tapping signal of the subject.

Series B.—After preliminary series *A* had been run off, the quantitative aspect of deceptive reaction times was still further isolated by substituting lists of two digit numbers for the words upon the cards presented to the subject. In addition to the directions of L. or R. and U. or D. which the subject might obey or reverse the experimenter now instructed the subject to add or subtract from each number on the list two, three, four or five. If the subject were determined to deceive upon any card he not only chose the column and direction opposite to the experimenter's instructions, but also

added when instructed to subtract and vice versa. By this method it was intended to eliminate all meaning from the symbols dealt with in the subject's mental process, and to isolate as far as possible, the factor of deceiving the experimenter by uniformity in the reactions required of the subject. Time was allowed between the instruction and the starting signal for the subject to perform the central process of reversing the instructions, and the watch was started simultaneously with pronouncement by the subject of his first result; so that reaction times recorded actually included the time used in nine additions or subtractions. The interest of every subject but one was intrigued to a high degree (as reported in introspection and as evidenced by behavior during the experiment). This condition was established largely by the aggressiveness of the experimenter and the consequent keen desire of the subjects to "fool" or beat him at the game of deception.

A total of eleven series of tests were recorded, one of the ten subjects taking part in two series. Each series of tests exclusive of preliminary series *A* included 40 sets, eight lists to the set, as above described, four sets being run through in about an hour. Every subject reported positively that he attained the desired attitude of deception and each number list was given four times, each time with a different digit to subtract or add, in order to exclude as far as possible any chance influence of combinations of figures peculiarly difficult to any individual subject. In the principal experiment total averages were made separately for all O. reaction times and for all C. reaction times of each subject. Comparative studies and further analyses of these averages were made, as will appear in detail hereafter.

III. Results

The results of this experiment disclose two distinct types of reaction time behavior during deception. First, the positive type was clearly evident in four out of ten subjects. This is the type, hitherto believed to be practically universal, which is characterized by increased length of reaction times during deception, with accompanying increase of mean varia-

tions. Secondly, a negative type manifested itself with equal distinctness in three out of ten subjects. In the cases of these three subjects reaction times during deception appeared to be consistently shortened with, however, no consistent accompanying effect upon mean variations. Thirdly, a mixed group appeared in the cases of four subjects who completely shifted from positive to negative types on different days, behaving consistently within the positive or negative types on each single day's work.

A. Positive Type

No detailed study of results to prove the existence of this type will be necessary in the present experiment, since the positive deceptive type is probably most usual, and has been sufficiently established by all previous experiments reported within the reaction-time field. Table I. gives a summary and analysis of the quantitative reaction time results obtained for the four positive type subjects in this experiment.

TABLE I

Subject	Total Average "C" Reaction Times	Total Average "O" Reaction Times	Positive Difference Between "C" and "O" Averages	M. V. "C" Reaction Times	M. V. "O" Reaction Times	Positive Difference Between "C" and "O" M. V.	Probable Error	Average Percentage Correct Judgments
A.....	10.8344	14.546	3.7116	1.572	1.8331	1.8331	.1906	81.89
D.....	4.6592	5.5605	.9013	.5192	.5882	.069	.0568	81.57
E.....	7.3848	8.5181	1.1333	.7727	.8118	.0391	.09072	67.42
S.....	10.48	12.2771	1.797	1.598	2.402	.804	.2163	75.00

Hitherto it has generally been assumed in the diagnostic experiments that the m.v. for the critical or deceptive words will be greater than that for the non-critical. In the positive type the results of this experiment substantiate those of the diagnostic investigators. In the case of all four of the subjects of positive type the average m.v. for the deceitful reaction times exceeds the average m.v. for the truthful measurements by a margin that is probably significant. This state of fact also has been well established in the literature.

The introspections of positive type subjects, when analysed to the lowest common denominator, show *fear* appear-

ing in many guises, and this report of fear at crucial points does not appear in the introspection of the negative type subjects. One subject is "afraid that he will obey the instructions of the experimenter rather than his own opposite setting." Another subject feels "withdrawal from the experimenter while attempting to deceive," "feeling of nausea," "shrinking feeling." Both seem palpable expressions of a real fear content. "Tension" is also introspected. This "tension" does not seem to be that indefinable attribute of feeling tone postulated by Wundt, but consists of a very concrete and often localized muscular tension, sometimes in the feet, in clenching of the hands, in throat muscles, breathing muscles, muscles of the mouth, and especially at the diaphragm. One subject described this tension as "a restraint across the ribs, like indigestion." "Excitement" is a second introspective element reported solely by positive type subjects. It is variously described as "nervousness," "exhaustive excitement," and "general emotionality." I shall defer any discussion of the exact nature of this element to the consideration in a later report of blood pressure results obtained in war work which seem to have significant bearing upon the much disputed tridimentional theory of Wundt. Let me anticipate, however, by stating that I take this excitement to be an awareness of the visceral expression of any of the three major emotions expressed in the sympathetic division of the autonomic nervous system or of any other emotion which has flowed over into that system. The confusion of motor impulses which is very evident in the behavior of positive type subjects, is described introspectively as "conflict of more processes during the deceptive lists." Auditory, kinæsthetic, and visual imagery of the experimenter's instructions recur in consciousness and the mind is reported to "become blank." Subjects introspect that, in the truthful reactions, the experimenter's directions reinforce the motor setting while, in the deceptive reactions, above mentioned, imagery tends to break it up. They "forget whether they are adding or subtracting."

The behavior of the positive type subjects, which was

carefully noted upon each record by the experimenter, tends to bear out the introspection of the positive type as reported above. The flush, traditionally indicative of guilt, is almost always present with the positive type. Frequent obvious confusions and embarrassments are noted together with evidences in facial expression of great effort, which effort, nevertheless, proves unsuccessful in precluding numerous clearly manifested inhibitions and interferences of motor impulses.

What, then, is the mechanism by which positive type reaction times are delayed? Jung and his school unanimously ascribed the increase of reaction times to emotion, and, from the above reported results, emotion of one sort or another would certainly seem to be the prime cause. We may go a step further, I believe, with the data in hand, and postulate *fear* variously expressed and always more or less Freudianly concealed, even in the introspection, as the prime cause underlying delayed reaction times in the positive type subjects. The witness, unable to concentrate because of the inevitable physiological expressions of his fear, becomes more and more introspectively aware of the fear content itself, and proceeds to exert great effort to suppress this fear, yet, since the only method of successfully suppressing emotion is to eliminate this emotion from the focus of consciousness by concentration, such increased effort only tends to bring intellectual and motor processes into consciousness in addition to the fear content already present. At this step, introspective awareness of the then conscious difficulties of performing the task in hand act as a new and adequate stimulus to a new set of fear reactions (which, of course, constantly enhance themselves through the adrenal autonomic mechanism). The final consequence, which is of course inevitable in any intellectual task performed under distraction, is a conflict and confusion of motor impulses directed to the work in hand, with prolonged inhibitions as the final step. In short, the positive type subject, engaged in the game of deceit, makes a tremendous effort to win, but, like the beginner at golf or tennis, he is unable to concentrate his attention upon the end to be

attained, with the result that consciousness of muscular movements and the general modus operandi precludes success and he misses the stroke. The positive type subject is the unsuccessful liar in the detection of whom trial lawyers and jurists have delighted through the centuries.[1]

B. Negative Type

Table II. shows that, taking the analysis of averages just as it stands, the negative type is precisely as clean cut and well-marked as the positive.

TABLE II

Subject	Total Average "C" Reaction Times	Total Average "O" Reaction Times	Negative Difference Between "C" and "O" Averages	M. V. "C" Reaction Times	M. V. "O" Reaction Times	Negative Difference Between "C" and "O" M. V.	Probable Error	Average Percentage Correct Judgments
C.....	9.85147	9.48308	.36839	1.6294	1.5693	.0600	.1831	78.67
G.....	11.4955	10.7975	.6980	1.1425	1.1764	.0339	.1837	75.00
K.....	8.4701	7.9361	.5341	.8794	.5716	.3078	.1017	75.00

A careful study of the individual data, together with a comparison of the averages for different days, shows, however, that the balance is much more unsettled. The negative attitude seems much harder to maintain. These conclusions are formed principally from the greater relative number of reversals of reaction time behavior; although unlike the reaction times of the mixed group no two of these reversals ever occur together, and in no instance does a reversal of type persist during an entire day's work. Transferring our attention, for the moment, from laboratory experimentation to ordinary everyday experience, I believe it will be commonly recognized that there are individuals among our acquaintance who can lie faster and more fluently than they can tell the truth. This type of liar never has been discovered in association reaction time experiments as reported in the literature, probably for the very simple reason that such individuals were wiped out in the general averages and left unaccounted

[1] It is of interest to note that the positive type give the most extreme and erratic blood pressure changes; but exhibit the least marked breathing changes, according to Benussi.

for when experimenters reported as their final net result a certain percentage of cases successfully detected by association reaction time tests. Indeed, in both the experiments of Washburn and Leach[1] and Yerkes and Berry[2] close inspection of the quantitative results will reveal instances of this unrecognized negative type. Under the conditions of the present experiment it seems clear that, the confusing and complicating factors of the equation having been eliminated, and the bare quantitative reaction time behavior alone having been retained, the negative type appears with unmistakable definiteness. Indeed, during the latter part of the experimentation, after this type had begun to manifest itself, the experimenter was able to bring his percentage of correct judgments from 32 per cent. to 75 per cent. in the cases of subjects *G* and *H*, merely by picking the faster reaction time lists as deceptive.

In two of the three subjects of negative type it will be noted from Table II. that negative m.v. differences of .06 and .3078 appear. Subject *G*, on the other hand, shows a positive m.v. difference of .0339. Just what value is to be placed upon these somewhat equivocal m.v. differences is rather problematical. In general, it would seem to show that while the positive type generally tends to show positive m.v. differences, the negative type is not so definitely correlated with any particular m.v. behavior.

Subjects of the negative type give very little introspective data indicative of fear. Occasionally the subject reported "worry" or some similar emotional state of which fear is probably an ingredient, but these elements were always reported in the fringe of consciousness only. Anger, on the other hand, was frequently introspected and seemed to be aroused in negative type subjects by the least difficulty in arithmetical processes during the deceptive lists. Negative type subjects reported that they felt a certain "tension" coming on (especially in the breathing muscles which would seem to bear on Benussi's results); but that they felt that

[1] *American Journal of Psychology*, 1910, pages 63–67.

[2] *American Journal of Psychology*, 1909, pages 22–37.

this tension would interfere with the speed of their reaction times and they, in consequence, were able voluntarily to relax. It is this relaxation which the positive type of subject never reports himself able to accomplish. No negative type subject reported "excitement." All, however, reported a definite "plan." This element is reported as persisting in the consciousness of negative type subjects both as a motor organization and as an intellectually retained concept and is an element entirely lacking in the deceptive consciousness of the positive type as reported. Both motor setting and concept are relegated to the fringe of consciousness when actual arithmetical processes commence, but certainly both seem to co-exist with the other elements during the entire deceptive consciousness. The plan seems to include: (*a*) plan to go slower on the truthful reactions and to welcome any difficulties which will lengthen these reaction times; (*b*) plan to relax muscular tension whenever it reaches consciousness; (*c*) plan to keep just within the maximum speed during deceptive reactions but not to try to exceed this rate lest subject should "break into inhibitions." In this connection it is interesting to note that both positive and negative types believed that deceptive reactions were much slower than their truthful ones. The negative type subjects reported a "tremendous feeling of effort with occasional necessity for voluntarily inhibiting persistent imagery of one sort or another which may disturb attention." This voluntary inhibition seems to be successful with the negative type as evidenced in the quantitative results.

The behavior of the negative type subjects, as noted by the experimenter, was almost wholly calm, confident, and showed a high degree of intellectual concentration. The guilty flush was almost altogether absent and the manner of the subject was usually more convincing upon deceptive lists than upon truthful ones. Occasionally inhibitions were noted but 95 per cent. of all such inhibitions occurred during the truthful lists, and as reported by the subjects' introspection seemed to be welcomed by the witness and even voluntarily prolonged. The experimenter frequently observed, however,

the tendency in negative type subjects to avoid the experimenter's eyes; and this symptom suggests that fear is present with subjects of the negative type also, although such subjects are successful in preventing the emotion from emerging into the focus of attention.

In the negative type we find the successful liar. The psycho-physiological mechanism by which reaction times are delayed with subjects of the positive type is voluntarily controlled and stopped at its inception by negative type witnesses, with the result that we have the intellectual task of sustaining deception at the very center of attention. The mind then being concentrated upon the end to be gained is able to use all its ingenuity and skill in deceiving the experimenter, and, since no mind is capable of exact judgments of time over a long series of reactions, the only flaw in the negative type's efforts at deception lies in said efforts being *too successful.* This, however, is a reaction time difference of much more unstable and nicely balanced character than is the positive type difference, since any temporary discomfiture of the negative type during deception or any chance impairment of general mental fitness will probably delay the deceptive reactions just enough to make them practically indistinguishable from truthful measurements. The negative type subject then is the one who, through the centuries, has had the joke on lawyers and juries.[1]

Table III. shows at a glance why further analysis of two apparently positive type subjects and two as obviously negative type subjects is deemed necessary. Each row of figures on Table III. represents one day's work and it will be seen that the characteristic which differentiates these four subjects from the general positive and negative types is the complete reversal of reaction-time behavior from one type to the other during an entire day's work. It is, of course, to be expected that occasional reversals of reaction-time behavior should occur and where such reversals occur normally scat-

[1] The blood pressure symptoms of the negative type show a more even curve but with just as great or even greater net rise in b.p. than is found in the positive type. According to Benussi the breathing of the successful liar shows greater modification than that of the unsuccessful.

tered through the general tests no significance could be attached thereto; yet, when the subject so completely shifts from one type to the other, that, after the first series of tests on the reversal days, the experimenter was able to attain a percentage of over 90 in his correct judgments of deception by postulating a shift in type, it seems valuable to further subdivide positive and negative type individuals who are subject to these clear-cut reversals. For the sake of convenience in reporting this further analysis the writer has called

C. Mixed Group

TABLE III

Mixed Group											
Positive						Negative					
Subject B			Subject F			Subject C			Subject I		
Number Sets per Day	Number Positive	Number Negative	Number Sets per Day	Number Positive	Number Negative	Number Sets per Day	Number Positive	Number Negative	Number Sets per Day	Number Positive	Number Negative
2	2	0	3	1	2	2	0	2	*2	2	0
2	2	0	4	4	0	4	0	4	*4	4	0
2	2	0	4	3	1	4	2	2	*6	6	0
*2	0	2	4	3	1	*3	2	1	*3	3	0
2	2	0	*4	1	3	4	1	3	4	1	3
*2	0	2	4	4	0	4	1	3	4	0	4
4	3	1	4	4	0	*4	3	1	4	1	3
*6	0	6	3	3	0	4	0	4	4	0	4
4	4	0	3	3	0	*3	2	1	4	0	4
4	3	1	*4	0	4	*4	4	0	4	1	3
8	8	0	*6	0	6	4	0	4	4	0	4
6	6	0	4	4	0	3	0	3	4	0	4

these four subjects a mixed group, although it should be understood that the name is not intended in itself to carry any significance. When the above-mentioned reversals are checked up with the behavior of the subject on reversal days, we find that on negative reversal days of positive subjects the physical and mental condition of the subject seems to have been exceptionally good, with frequent evidences of the subject's being in good spirits, or particularly happy over some occurrence in his personal affairs. Also, behavior on these reversal days seems correspondingly to

shift to negative type behavior, carrying with it a change in the general tone of the introspection. In short, it appears that when these positive type subjects were in an exceptionally favorable physical and mental condition they were able to attain the successful poise of the negative type in deception. Again, it will be noted in Table III. that subject *I* shows a marked positive reaction time type for the first four days of experimentation, and that thereafter he attained the negative speed of reaction and held it to the finish. This seems clearly to suggest preliminary practice with resultant skill in the game of deception. It is also to be noted that subject *C* reported himself "fatigued or physically tired" on every day when his reaction times lapsed into the positive type.

TABLE IV

Mixed Group					
Type		Positive		Negative	
Subject		B	F	C	I
Totals of reversal days	Average "C." lists	9.0781	13.893	8.175	6.658
	Average "O." lists	7.8844	12.1135	8.76875	8.7709
	Difference	−1.1938	−1.7807	+ .5938	+2.1129
	M. V. "C." lists	1.1889	1.95	.972	.996
	M. V. "O." lists	.9718	1.1188	2.322	2.123
	Difference	− .2171	− .8312	+1.350	+1.127
Totals without reversal days	Average "C." lists	7.308	11.848	9.5416	6.7125
	Average "O." lists	8.492	13.307	8.3887	5.4125
	Difference	+1.1839	+1.459	−1.1529	−1.3
	M. V. "C." lists	.8733	1.502	1.2016	1.0764
	M. V. "O." lists	1.2539	2.427	1.1693	1.0292
	Difference	+ .3806	+ .925	− .0322	− .0472
Total of all days	Average "C." lists	7.7014	12.096	9.38319	6.903
	Average "O." lists	8.3569	13.162	8.43219	6.234
	Difference	+ .6556	+1.066	− .951	− .669
	M. V. "C." lists	.9434	1.556	1.1754	1.0764
	M. V. "O." lists	1.1912	2.269	1.3011	1.0292
	Difference	+ .2912	+ .713	+ .1257	− .0472
	Average per cent. correct judgments	81.61	78.03	80	78.83
	Probable error	.1061	.1828	.1340	.1326

Table IV. shows evidence, in further analysis of reversal day averages as compared with total type averages for above four subjects, tending to indicate sharp contrasts in quantitative measurement between reversal day and general type

reactions. Of course these reversal averages could be left buried in the general quantitative average, but above further analysis shows the sharp distinctness of the averages of the unified groups when same are separated. In general the study of Table IV. is intended to contrast the two markedly opposite statistical tendencies of the reaction times of mixed group subjects. It may be noted that both positively inclined subjects in the mixed group show positive m.v. differences for their total average m.v., while negative m.v. differences appear for the negative reversal groups. The two negatively inclined subjects of the mixed group, moreover, show negative total m.v. differences, with positive m.v. differences for positive reversal groups. The writer does not attach any great significance to this m.v. as a criterion, since the actual differences were very small and would be very hard to follow up in any practical test. It is of interest, however, to note that, other factors being equal, the m.v. differences seem to follow the general type of the subject.

It appears to the writer that the mixed group probably takes in the whole middle range of liars. Only very good liars succeed in keeping every single day's work up to negative type deception speed; and most negative type liars probably have their "off days" when they are unable to command their attention and mind to the high degree necessary to shorten the reaction times during deception. On the other hand it is a very poor liar who is never able to attain sufficient concentration to throw himself for a day into the class of successful deceivers. It would seem to the writer that, after practice in deception was eliminated, the predominant type of any subject could easily be detected by a sufficiently long series of measurements; and it may be said that the experimenter in the present experiment, after once determining the general type of a subject, found reversal days exceptionally productive of accurate judgments because, during such reversals, the unnatural tendency of the subject seemed to be exaggerated and very consistent.

Psycho-legal Conclusions

It seems to the writer that this experiment has added one stone to the foundation necessary to qualify the psychological expert on deception before the court. The district attorney points to the flushed face and obvious confusion of a witness for the defense; but our psychologist is put on the stand to testify that he has proved the defendant to be of negative deception type. Attorney for the defense then can place before the jury the psychological fact that, were a negative type witness lying, no such flushed face and obvious confusion would appear, for the negative type witness is the gifted liar who would be expected to exhibit much less confusion were he lying than if he were telling the truth. Or our expert may succeed in discrediting the star witness for the state solely because said witness, being a subject of negative reaction time type, was oversuccessful in going through a terrific cross examination with unbroken rapidity and consistency. The stress and high tension of the layman's mildest experience in court, either as witness or as a criminal defendant, should, I believe, tend to keep the individual well within his predominant type; yet our knowedge that there exists a mixed group subject to sharp reversals of reaction time behavior, should lead the boldest psychological expert to check up his reaction time results with other test data before venturing an opinion in court. In any event, if psychological authority can succeed generally in disabusing the minds of the average jury of the conviction that the slightest hesitation, delay, or confusion in the testimony of any witness is indicative of deception, or that rapid and clever answers in cross-examination are indubitable earmarks of a white conscience, a constructive development in the weighing of evidence should be effected in an innumerable number of trial cases.